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THEORIES OF INTELLIGENCE*

By L. L. THURSTONE
THE UNIVERSITY OF CHICAGO

EVERY one of us judges daily the intelligence of other people, and occasionally we even estimate our own. It is a touchy matter, for we would rather have our integrity put to question than our intelligence, and yet the fact is no doubt generally acknowledged that intellectual endowment is unevenly distributed in the population, that some men are brighter than others.

The nature of human intelligence has been a problem for centuries. Until about sixty or seventy years ago this problem was discussed entirely at the verbal and speculative level. A pre-experimental history of this subject would reveal a great deal of speculation in which we would find much ingenuity and insight. It would be an unstable guide for scientific work, however, because here as in other prescientific speculation there was more error than fruitful insight.

The first sustained attempt in modern times to appraise the intelligence of individuals as distinguished from examinations for scholarship or proficiency can be credited to Sir Francis Galton (1822–1911). He attempted by experimental methods to ascertain the differences among persons as to their mental endowment as distinguished from the estimation of proficiency. In 1885 he started a laboratory in London where early attempts were actually made to appraise individual differences. At that time Galton limited himself mostly to sensory and perceptual functions, which were not successful as indices of intelligence. Many investigations have been made to ascertain the relation between various sensory functions and human intelligence, and the findings have been definitely negative. In a later paragraph I shall return to the sensory functions in their dynamic aspects.

The problem was next tackled in some extensive studies of school children. These studies were motivated by the hypothesis that differential rates of progress of children in the schools were due largely to differences in mental endowment and that children should be classified for school work according to some index of intelligence. This problem called for an estimate of the mental alertness of each child which should be based on evidence as far as possible independent of formal school progress. It was recognized, of course, that a child's school progress was determined only partly by native endowment and partly by motivational conditions in his home and school environment. The independent appraisal of a child's mental endowment would be successful to the extent that the effects of nature and nurture could be analytically separated.

* This paper was read before the Chicago Literary Club on February 12, 1945.

The best known among the early methods of appraisal was the Binet test. The plan of this test was simple. Each child was asked a number of questions over a fairly wide range of content. The questions were intended to reveal the child's ability to solve simple problems with content that was drawn from his environment. Much of the Binet test depended on the assumption that, other things being equal, a bright child would gather more generally available facts and ideas about his environment than his less-gifted classmates. Each problem or test item was given to children of different ages, and it was experimentally determined at what age one could expect a half or two-thirds of the children to do the task correctly. The Binet tests have been translated into many languages. For each translation it has been necessary to modify the content in order to adapt the test to cultural differences.

The total performance of a child on a Binet test is stated in terms of his so-called mental age. A mental age of 10 years, for example, represents merely the average performance of 10-year-old children and similarly for the rest of the scale. If a child attains a mental age of 10 and if he is actually 8 years old then he is two years accelerated. Children are frequently described in this way as to their degree of mental acceleration or retardation, and this classification is of practical value in the schools.

It has been found that there is a reasonably good correspondence between the degree of acceleration and retardation of a child and his school progress, but it must not be assumed that this correspondence is perfect. The correspondence is, however, sufficiently close to make the test appraisal of considerable value in dealing not only with groups of children but also with individual problem cases. For example, when a child fails to participate in a class he is ordinarily judged to be lacking either in ability or in motivation. If it is found by the Binet test that he is accelerated in intelligence his poor performance may actually be attributed to boredom. By advancing such a child he is put in a situation that is sufficiently challenging for him, and he may again hold his own. Unfortunately, this is the exceptional type of case.

When we examine the nature of the Binet test as to the content of each question we find that it is a hodgepodge. We find questions about vocabulary, others about simple arithmetic problems, puzzles of various kinds, simple reasoning problems, and the interpretation of proverbs. One of the ways in which a child's performance is analyzed is to study what is called scatter. Some children hold their own in all types of items, while others go much higher in one type of item than in other types. It is to be expected that some children will be accelerated in vocabulary, for example, and retarded in numerical thinking, or vice versa. This phenomenon of scatter in the performance on general intelligence tests is indicative of the fact that intelligence is a complex rather than a single trait. But in spite of the crudeness of the Binet test and other similar tests of general intelligence they have been of great practical importance in the schools.

We turn now to a later stage in the history of this problem, namely, the analysis of actual test performances. If two tests of intelligence are given to a group of people you might expect that those who excel in one of the tests would excel in the other test. This would be the case if the two tests were measures of the same trait and if the results were otherwise unaffected by experimental error. The degree of association between two variables is represented in statistical work by the coefficient of correlation. This is a numerical index which takes possible values between $+1$ and -1. If two measures have a correlation of $+1$

there is perfect association between them and accordingly all the individuals in the experiment would be arranged in exactly the same order by both tests. If the correlation is -1 then there is perfect inverse relation, and the top man on one test is the lowest man on the other test. If the correlation is 0 then there is no discernible correspondence between the two measures. They are then just as independent as if both sets of numbers had been obtained by tossing coins. Correlational theory was started by Galton in a more or less descriptive manner, and it has been very highly developed by the mathematical statisticians. As an example of the meaning of correlation we might consider the well-known association between height and weight. Men who are tall also tend to be heavier than those who are short, but the association is of course not perfect. If we were to compute a correlation for height and weight of a random sample of several hundred adult men, we should find that the correlation is about $+0.5$. Correlation theory is involved in the application of statistical methods to a variety of problems, and some of these methods are also applicable to the present problem.

At the beginning of the century quite a number of studies were made to determine this quantitative index of association between abilities to do different kinds of tasks, and it was soon found that the correlations between various tests that were supposed to be indices of general intelligence were far from perfect. Two inferences were drawn from these simple observations. First, it was concluded that no test is in any sense a pure measure of the postulated general intelligence and, second, that an appreciable part of a test performance is subject to fortuitous experimental error. At this time there also began to be considerable speculation as to whether general intelligence could be postulated as a single general function. The alternative was to consider intelligence as a complex of many distinct abilities. A third possibility which has some defenders is that intelligence is determined by thousands of factors that function without any pattern or groupings.

It was in 1904 that the British psychologist Spearman wrote a simple but epoch-making paper on the relations for groups of psychological tests that had been given to the same individuals. He found that under certain circumstances, which he specified, these correlations did indicate the existence of what he called a single intellective factor which he denoted by the letter *G*. This was the starting point for a series of lively controversies which are still current in more modern form in the British and American psychological journals, forty years after Spearman's provocative paper.

Until about 1930 the central theme in these controversies was nearly always the question whether Spearman's general intellective factor did exist. It was soon recognized that even with the best available controls the postulated general intellective ability of Spearman was inadequate to account for observed relations among experimental tests. It was found necessary to acknowledge the existence of other abilities in addition to a general intellective factor, but these were frequently referred to as disturbers of the fundamental relations of Spearman. During this time there was general acceptance of special abilities in addition to the factor *G*. The main scientific interest, however, was directed at the question whether general intelligence should be postulated in addition to the special abilities which disturbed the simple relations of Spearman's hypothesis.

In 1930 investigations were begun with a different emphasis. Instead of asking whether the experimentally observed relations among the abilities, represented by a series of tasks, could be accounted for by a single intellective factor, the

question was asked how many factors or abilities were implied by the observed relations. It was then left as a question of fact whether one or more of the abilities that were identified might turn out to be more general or central in character than the other abilities. For any given set of tasks that were given experimentally to a group of several hundred people the question was then to determine how many abilities were represented by these tasks and, further, to identify the nature of these abilities. Before the analysis of such experimental data could be undertaken it was necessary to extend the earlier methods of Spearman for a single factor to the n dimensional case for any number of factors. This work has progressed for a number of years with the assistance of some British and American mathematicians and physicists who have taken an interest in the formal aspects of the problem. During the past ten years the methods so far developed have been applied to experimental data with findings that are of both theoretical and general interest.

The first major experiment with the new n dimensional methods of factor analysis was started in 1934. A battery of 56 psychological tests was designed especially for this study. These tests were devised so as to represent a wide variety of tasks which had been represented in previous studies of intelligence. Included in this battery were tests which called for verbal comprehension, verbal reasoning, various types of fluency, speed in simple numerical work, quantitative reasoning, various forms of induction, verbal, visual, and auditory associations, visualizing flat figures and solid objects, various forms of abstraction with verbal, numerical, and visual material, reasoning about mechanical movements, and memory for different types of content. This battery of 56 tests was given to several hundred student volunteers, requiring about 15 hours of work for each subject. When the records had been assembled the correlation was determined for each pair of tests in the whole battery. This required the calculation of about 1,600 coefficients of correlation. For each pair of tests the correlation tells us the extent to which those who succeed in one task tend to succeed in the other.

It is an old observation about intellectual tasks of all kinds that the correlations are all positive. If two widely divergent mental tasks are considered, the correlation between them may be low but the association is always positive. In fact no negative correlations have ever been found for intellectual tasks. There is a rather common misconception about these relations. It is not infrequently asserted that those who are superior in one intellectual task are somehow inferior in some other intellectual task. Among students such an impression is not uncommon as regards linguistic and scientific abilities. The fact is that no negative correlations for performance in school subjects or in intellectual tests have ever been found.

A large table of correlations among these 56 tests constituted the starting point for a multiple-factor analysis with the new n dimensional methods for the purpose of discovering how many abilities must be postulated in order to account for the observed correlations. It was found that 12 factors were sufficient to account for these relations among the 56 tests. Those who are mathematically inclined may be interested to know that the rank of the matrix of correlations is the number of linearly independent factors that must be postulated.

It is a curious circumstance that in the multiple-factor methods one can determine the number of factors involved in a set of experimental tasks before the nature of these factors is known. The next problem is to determine just what these factors or abilities are like. Here we come to an interesting indeterminacy the

nature of which can be represented in simple form in a two-dimensional diagram. If we plot a number of points on a diagram we ordinarily assume that the x and y axes are given. In the factor problem the configuration of points is given, and it remains for us to insert the x and y axes. These axes represent the abilities or factors, and the problem is to locate them in a configuration so as to give scientifically fruitful interpretation of the test relations. If we are dealing with a number of points on an ordinary diagram and if these points arrange themselves in two streaks from the origin, then the location of the two axes can be easily chosen by simple inspection in such a way as to give the most parsimonious interpretation. In dealing with an n dimensional configuration it is necessary to do these simple things analytically because unfortunately we cannot make physical models in more than three dimensions. If it were possible to make 12 dimensional models so that we could look at them, then the factor problem would be quite simple. Since we are living in three-dimensional space and since the factor problem involves relations among many more factors or abilities, it is necessary to handle the problem analytically, but the principle is fundamentally the same as in the two-dimensional diagram.

In choosing a set of 12 reference abilities for the experiment with the 56 tests we adopted what we have called the simple structure principle, the nature of which can be explained without reference to its mathematical form. Let us suppose that there are distinguishable mental functions in overt performance which are caused by physically differentiable functions in the organism. It is not necessary to postulate that each mental ability is represented by a separate organ or that it has a locus in the nervous system. Some mental abilities may be so determined while others may conceivably be accounted for physically in terms of parameters of the system as a whole. This is a question about which the factorial methods make no assumption whatever. All that we are doing is examining the overt performances of several hundred people in a wide variety of tasks to determine how many differentiable functions must exist in order to produce their observed differences. Let us suppose that there are differentiable functions such as number facility, one or more types of memory, one or more kinds of verbal ability, facility in visualizing, facility in auditory imagery, and so on. Let us assume that these abilities operate to produce the performance in each test. Now it would certainly be very unlikely that all these abilities would participate equally in every one of the tests. If we knew what these abilities were and if we were asked to assemble 50 psychological tests of different kinds so that every test would call for every one of the mental abilities we should probably find it impossible to design such tests. In assembling the tests that are feasible and that represent intellectual functions of various kinds we are probably necessarily featuring some of these abilities in each task, while other abilities are almost absent. If there is an ability that is characterized by facility to deal with numbers, for example, then such an ability would not be called for in doing verbal tasks or visualizing tasks which have no numerical or quantitative aspect. Facility in auditory imagery, for example, would surely be absent in the visualizing tasks. The selection of a set of fundamental reference abilities is then the problem of choosing these abilities so as to simplify as far as possible our comprehension of each of the tests. We account then for each test by the smallest possible number of abilities. This is the simple structure principle which can also be stated in geometrical form. The geometric analogue for this principle is to

locate the x and y axes and all the other axes according to the most simplifying planes in the configuration of test vectors.

Applying this principle that we have called simple structure we find that some of the abilities stand out rather clearly and can be easily interpreted. In exploratory studies we always find some factors whose interpretation is obscure. It is encouraging, however, that when experiments are carried out on groups of people who differ in age and education and with new sets of tests we have succeeded in identifying the same basic mental abilities. It is this kind of invariance which makes the search encouraging. It has been found also that factors which are obscure in an experimental study become clarified in subsequent studies with tests that are especially designed to investigate each domain.

When the analytical results have been obtained the final interpretation consists in discovering the nature of the ability that has been shown to be present in one group of tests and absent in the other tests. Sometimes we have rival hypotheses about the nature of an ability which has been shown to exist. Fortunately such questions can be resolved as questions of fact, and so we are not dependent on debating-society methods for settling such questions. We proceed then to design new tests which are crucially differentiating for rival hypotheses about the nature of a factor. The new tests go into the next factorial experiment, and we can then usually discover which of the two hypotheses is correct. We must acknowledge, however, that sometimes the subsequent factorial study shows that both hypotheses were wrong. Such a situation simply means that we must guess again.

We turn next to a brief description of each of the mental abilities that have been identified so far.

One of the abilities that have appeared very clearly in a number of independent investigations is the ability to visualize solid objects. This ability is known as the space factor and it is denoted by the letter *S*. It represents the ability to visualize flat figures or solid objects. As far as we can determine at present this factor covers figures in flat space and solid objects in three dimensions. At first we were prepared to find separate factors for these two types of thinking. This space factor is not new. It is undoubtedly the same visualizing ability that was described by Galton in his informal studies of imagery types. He differentiated between visual and auditory types. In any audience of educated men there will be a considerable proportion who rather dislike tasks that require visualizing and others who depend on it as their principal medium of thinking. We are dealing here with differences among persons in the readiness with which they manipulate different types of imagery. A man's style of writing and his preferred methods of exposition are likely to reveal his preferred imagery.

A number of tests are available which give fairly good indices of the space factor *S*, and their application in employment selection and in student counseling seems rather self-evident. For example, if a boy is a poor visualizer as determined by these tests it is a safe bet that he will not be happy as an apprentice in a drafting room, and it is also equally safe to predict that he will not enjoy descriptive geometry and similar subjects. It is known that the space-factor is heavily involved in mechanical aptitude, but it is only one of the components in mechanical aptitude.

Another factor that is quite easily identified has been called the number factor *N*. This is represented by facility in doing simple numerical tasks, but it must not be inferred that this factor is heavily involved in arithmetical reason-

ing or in mathematics. One should not be surprised to find some very competent mathematicians who are not high in the number factor *N*. On the other hand, one should expect to find a good cashier or bookkeeper to have facility in this factor. Arithmetical reasoning, which is represented by the familiar statement problems in arithmetic, involves a number of other factors often more important than number facility *N*. In designating this factor as concerned with numerical thinking we have been puzzled about a closely related problem. If we are here dealing with differences in native endowment, then it has seemed to us that each factor should be described in terms of the kinds of thinking and imagery rather than in such terms as *number* which is a cultural concept. Attempts have been made to interpret this factor in some more fundamental form. Eventually, when the nature of this factor is more completely understood, it may be possible to appraise it by tests which are nonnumerical in character. It might then be merely a historical accident that the factor was identified in tests that happened to be numerical in character. On the other hand, there is a possibility that there exists a distinguishable mental ability which is definitely related to numerical thinking as such. These are questions for the future to answer.

One of the socially most important factors that have been identified is called verbal comprehension *V*. It represents facility in dealing with verbal concepts. It is represented rather well by tests of vocabulary or tests of verbal reasoning, as in the interpretation of proverbs or the comprehension of difficult prose. Our educational system is built around this medium, and consequently those individuals are generally judged to be intellectually retarded who are deficient in verbal facility even though their other intellectual powers may be exceptional.

Investigations that have been completed thus far have revealed the existence of three or four verbal factors, but only two of them are at present understood. In addition to the important verbal comprehension factor *V* there has been identified another verbal factor which has been called word fluency *W*. At first it was difficult to see just why these factors, which are both verbal, should nevertheless be so clearly separated in the factorial analysis. Closer examination of the tests by which these two verbal factors were identified revealed an interesting difference. The tests which are heavily saturated with the word-fluency factor *W* have this characteristic in common that the individual subject must produce his own words in a restricted context, whereas the verbal-comprehension factor *V* requires that he understand the verbal material that is given to him. A simple test for the word-fluency factor is to ask the subject to write, for example, as many boys' names as he can think of in two or three minutes. The nature of the task can be varied considerably with the same results. The subjects might be asked to write a list of things to eat and drink. The subject who is gifted in word fluency will keep on writing, whereas a person who does not have this kind of fluency will write perhaps a dozen words and then have a block. With some encouragement he may proceed with another spurt, and after a few seconds he will have another block. These two verbal abilities are distinct in the sense that a man may be verbally fluent even though he has a limited vocabulary, and another may have an extensive vocabulary and be capable of profound verbal comprehension even though he is not verbally fluent. The social and educational implications of these differences are self-evident.

When reading a treatise by Weisenberg and McBride on aphasia I became interested in the verbal tasks that some

of the patients could do and the other verbal tasks they could not do. The differentiation between these two types of verbal tasks for one group of aphasic patients seemed to be well described in terms of the two verbal factors *V* and *W*. This particular group of patients failed on all the tests which involved the word-fluency factor *W*, but they seemed to be able to do the verbal tasks in which the verbal-fluency factor *W* was absent. This factorial differentiation among verbal factors is worthy of further study to ascertain whether the factors that have been identified on normal adults can be identified perhaps even more clearly in some types of speech pathology. There is indication at the present time that the word-fluency factor *W* may be a complex and that it can be broken up into further factors. It will be reassuring if different approaches to the same problem should agree as to what constitutes the differentiable verbal abilities.

It is a commonplace observation that some persons have good memories and that others have poor ones, and it is also a commonplace among the current psychological textbooks that memory is not a separate mental faculty. The result of factorial studies so far seems to sustain the popular impression rather than the current psychological dogma. There seems to be good indication factorially that people do differ in memory quite apart from their other mental abilities. A man who is of superior mental endowment may or may not be fortunate enough to have a good memory. The memory factor that has been identified seems to be represented best in the ability to remember paired associations. There are indications of other memory factors that may be concerned with memory for temporal sequence as distinct from paired associations, and there probably exists also a separate ability for what is called incidental memory, namely, the ability to recall past experience without previous intention to recall it. If this differentiation should be sustained, then we should distinguish between the ability to memorize intentionally and the ability to recall past experiences with no intent to recall them. The memorizing factor seems to transcend the nature of the content and so becomes applicable to numerical, verbal, or visual material. A person's performance in memorizing material of different kinds will, of course, be affected also by his abilities in the imagery types that are specifically involved.

One of the most interesting of the primary mental abilities is the ability to discover the rule or principle in the material that one is working with. This primary ability has been called induction. It definitely transcends the nature of the material, and consequently it must be centrally determined in some way. It does not seem to be directly associated with any of the sensory modalities. It is only natural that one should ask the question whether originality and creative ability are represented in this factor, but on that question we have so far no conclusive evidence. Our guess is that facility for induction in the sense of discovering the rule or common principle in the material with which one is working is not the same as originality and creativeness. It would certainly be of great value to discover any dependable indicator of creative talent, but on that problem the factorial studies so far completed can only give us a few clues.

Since induction has been identified as a primary mental ability it is only natural to look for deduction, and it has also been found to be a primary factor, but it has not been identified with sufficient clarity to be represented in the psychological tests that are recommended for use in the schools.

There has appeared in several studies a factor which we have not attempted to name but which is related to these rea-

soning factors. The best that we have been able to do so far is to associate it somehow with the ability to carry out restrictive thinking. By this we mean the ability to carry out a reasoning problem in which there is a definite answer as distinguished from those tasks in which the effective solutions are not unique.

In the past few years a different type of factorial study has been carried out on what we have called perceptual dynamics. In early work in this field it was found that sensory acuity was no index whatever to the mental abilities of the subject. In returning to the field of perception we have been primarily interested in the dynamics of perception. By this we mean apparent movement phenomena as studied by the *Gestalt* psychologists and other visual and auditory effects most of which have a temporal aspect. Five of the factors revealed in the perceptual functions are concerned with speed. These are: reaction time, speed of perception, speed of judgment, speed of closure, and rate of reversals in ambiguous perception. In addition to a factor concerned with speed of closure there has been identified another which has been called flexibility of closure. These two closure factors are of special interest because they are probably associated not only with intellectual types but also with temperamental characteristics.

The phenomena of closure can be illustrated by a simple example. If you see a word in which a part of each letter has been removed you may look at the word as if it consisted of a group of unrelated spots. You may look at it that way for several seconds in the attempt to put meaning into it. You may suddenly see what the word is. That would be an example of closure. The apparently chaotic and unrelated spots suddenly become unified into a single percept. You then see one thing instead of many disparate things. The readiness with which people get closure seems to be a trait much more fundamental than appears at first sight. It is not merely the ability to solve a trivial puzzle. It seems to extend to much more important tasks. A group of administrators who were examined in Washington in the attempt to study their mental characteristics did reveal among other things that the successful administrators scored well on certain tests of closure. A descriptive interpretation might be that they are men who readily unify the apparently disparate elements in the work for which they are responsible.

Another closure factor has been called flexibility of closure, and this factor may be socially just as important as speed of closure. Wertheimer has described problem-solving as the ability to destroy one configuration in favor of a better one. This is a good description. It implies the ability to destroy or ignore one configuration of things in order to see the same things in a different configuration. Since these two closure factors, which have been called speed and flexibility, have only recently been identified, we cannot speak with confidence about their implications. They are known to be distinct from the visualizing or space factor *S*.

The question has been raised about the primary factors that have been identified, whether any of them represent a modern form of the central intellective factor that Spearman postulated forty years ago. If we were to make a guess in answer to this question we should probably consider the inductive factor and the two closure factors. But the solution to the problem of the central intellective factor is probably more indirect. It has been found that when a group of individuals are appraised as to each one of the primary mental abilities that we know something about now, the primary abilities themselves turn out to be correlated just as the original tests were correlated.

In other words, those who have some of these abilities tend somewhat to excel in the others, but the associations are not close. In dealing with individual cases we cannot infer, for example, that a man has good number facility just because he is a good visualizer. But one can say that those who excel in any one of these primary abilities tend to be better than average in the other abilities. This raises the question about why these primary factors are correlated. Our present interpretation is that there exists what we have called a second-order factor which is more fundamental than the primary. If this central factor exists, then it facilitates the function of all the primary or special abilities. Our present interpretation is that the central intellective factor which Spearman postulated exists in the form of some central parameter which has a positive influence on all the special mental abilities we have been discussing. It has been found that some of these primary factors are more heavily saturated with this central factor than others. Rote memorizing ability has very low saturation on the central intellective factor, whereas induction and verbal comprehension and visualizing have rather high saturations on the central intellective factor.

Our conclusion regarding this old question is then briefly as follows: There seems to exist a large number of special abilities that can be identified as primary abilities by the factorial methods, and underlying these special abilities there seems to exist some central energizing factor which promotes the activity of all these special abilities.

In addition to the studies on adult subjects the same type of analysis has been made on experimental data for high-school children and lately on a group of several hundred 5- and 6-year-old children. Special tests had to be designed for the kindergarten children who were not yet able to read. Essentially the same primary mental abilities have been identified with these young children as with the adults. It has been found also that some of these factors such as visualizing and closure seem to mature very early. It is possible to determine at the age of four or five whether a child is a good visualizer, and we are certain that young children are much more clever in reasoning than is ordinarily believed. The work with young children is motivated partly by the thought that methods of instruction may eventually be adapted to the mental profile of each child. It is well known that the public schools are full of reading-problem cases. These are children who seem to have some mental ability but who are slow in reading. It is conceivable that different methods of teaching should be used with children of different mental profiles.

Finally we turn to the question of how the appraisal of special tests for each primary mental ability can be made practically useful. Instead of attempting to describe each individual's mental endowment by a single index such as a mental age or an intelligence quotient, it it preferable to describe him in terms of a profile of all the primary factors which are known to be significant. A glance at such a profile shows whether a man is generally gifted, whether he is exceptionally gifted in one or two of the primary factors, or whether he has some conspicuous limitations. The tests which have been made available for use in the schools represent six of the primary factors whose implications are already fairly well indicated. As new factors become isolated and interpreted as to their implications, they will be added to the tests which are made generally available in schools and eventually for employment selection. If anyone insists on having a single index such as an I.Q., it can be obtained by taking an average of all the known abilities. But such an index tends so to blur the description of each

man that his mental assets and limitations are buried in the single index. It seems better to use a profile and to extend it with new factors as they become isolated.

The question is often asked whether these primary factors are inherited and whether they can be trained. A study has been made of 150 pairs of identical and fraternal twins in Chicago, and it has been found that the identical twins are much more alike in each of the primary mental abilities than are fraternal twins. This finding is what one should expect, and it agrees with previous studies on twins with less differentiated measures of mental endowment. The interpretation of these genetic studies is that inheritance plays an important part in determining mental performance. It is my own conviction that the arguments of the environmentalists are too much based on sentimentalism. They are often even fanatic on this subject. If the facts support the genetic interpretation, then the accusation of being undemocratic must not be hurled at the biologists. If anyone is undemocratic on this issue it must be Mother Nature.

To the question whether the mental abilities can be trained, the affirmative answer seems to be the only one that makes sense. On the other hand, if two boys who differ markedly in visualizing ability, for example, are given the same amount of training with this type of thinking, I am afraid that they will differ even more at the end of the training than they did at the start. Both of them would no doubt improve in overt performance, but the boy with native ability would probably outdistance his less gifted partner.

In comparing the profiles of students with their known professional interests, one finds that students who are highest on the two verbal factors are usually interested in some linguistic occupation, such as writing or journalism. Combined with the reasoning factors the profile indicates an interest in the more formal aspects of language. A combination of the reasoning factors and the visualizing factor is characteristic of students of physical science and engineering. So far we have not seen any characteristic profile for medical students. This probably means that the medical profession is so broad in scope that it has opportunity for wide range in types of talent. We have seen some very striking examples of children in the Chicago schools whose special abilities were unknown until they were examined by tests of the primary factors. One child was recently brought to our attention who scored extremely high in the verbal factor *V* and low in everything else. He is a tremendous reader and yet he fails in all his school subjects. He does not seem to be able to reason with what he reads. Another child was extremely high in word fluency and low in all other factors. This child has a reputation for his great facility in fabricating stories to get out of one delinquency after another. Another child was considered to be a dunce until it was found that he was the best visualizer of his age but he was low in the verbal factors. This was a clue not only for the education of the child but also for directing him into something where he might have prestige. Such a resolution of his problem solved also a personality difficulty because he can now identify himself with something in which he excels.

Studies must be undertaken as soon as possible to investigate factorially such fields as mechanical aptitudes, musical talent, and artistic talent. It is often supposed, for example, that mechanical ability is nothing but manual dexterity, but such is not the case. Mechanical ability is mostly in the head. It is a complex of abilities whose isolation should be of considerable importance for science and industry. That is still to be done.

The multiple-factor methods that have been developed in the past decade have just begun to produce results. We have barely started in a field of investigation that will enable the next generation to be more rational than we have been in planning the education of their children and in selecting people for each kind of work. Even at the present time psychologists can do much more effective work in appraising mental abilities than was possible ten years ago.

This work is consistent not only with the scientific object of identifying the distinguishable mental functions but it seems to be consistent also with the desire to differentiate our treatment of people by recognizing every person in terms of the mental and physical assets which make him unique as an individual.

THE PATH I CHOOSE

What is this freedom which men boast they own?
My barque seems free upon the waters,
Yet no matter what the course I set
I only partly guide.
You, like a lodestar, turn my compass north
Across the trackless miles,
A feeble force but constant
And a source directive in my better hours.

For life is unknown ocean,
And winds may blow destructive rancor to my soul
Or anchor it becalmed on surface of a liquid glass
To shrivel 'neath the blazing sun.
At other times I race before a cooling draft
And speak of progress toward a port.
The tide may raise my song to buoyant tone
Or wring a minor wail at its recess,
And, under all, the oceans move in vaster whirls.

Men speak of final ports. This craft was ocean born
And knows no rest.
If port there be, it yet has sighted none,
But this is life, not yonder, and the best we have
Lies in this frame and not in distant scene.
Let me speak fair to all of those who pass,
Hail and farewell or traveling abreast.
May I no wreckage cause nor halt the travelers fast
Who know where they are bound.

Yet wandering to touch at many strands,
The known and loved, the new uncharted isles,
I seek the gems of each and hope to land
Upon your restful shore before the end.

JOHN G. SINCLAIR, 1937

SCIENCE IN THE POSTWAR LIBERAL ARTS PROGRAM*

By LLOYD W. TAYLOR
PROFESSOR OF PHYSICS, OBERLIN COLLEGE

COLLEGE teachers of science quite properly are concerned with whatever threat to the liberal arts exists in the recent diversion of our educational resources to wartime training. Perhaps the most disquieting aspect of such a threat was the war's acceleration of a trend that was becoming pronounced long before 1941, the trend toward ever more narrow specialization. Even associated departments were having increasing difficulty in understanding each other's language, and communication between curricular divisions had completely broken down. This trend, growing in large measure out of the nature of graduate training for the doctorate, is probably unavoidable in a university setting, but it has been planting the seeds of destruction of the liberal arts ideal in our colleges.

One of the fruits of this Balkanization of the curriculum is a resuscitation of the old antagonism of the humanities toward the natural sciences. Vestiges of this were in evidence before the war. Penalizing science teachers by requiring two or three clock hours of laboratory duty for one teaching credit hour was one manifestation of it. Penalizing students for taking laboratory subjects by trying to retain the old scheme of laboratory fees was another. But these were mere vestiges of an old Pharisaism. Now, as one of the consequences of the war, this attitude has been fanned into new life, and I fear that we shall have some of the old battles to fight over again, this time with certain handicaps which we did not have before.

The humanities are now definitely alarmed. Profoundly mistaken, they are nevertheless thoroughly convinced that increased interest in the sciences, stimulated by scientific contributions to the war, will act to the disadvantage of the humanities. Professional journals are full of articles to this general effect. One can almost say that the academic equivalent of mob spirit is being aroused, with a view to tarring and feathering the sciences and riding them on a rail out of the liberal arts.

To keep the record clear I am sure that it is accurate and appropriate to say that the interest of science teachers in this problem arises not out of fear about continuation of the sciences but out of genuine concern for continuation of the liberal arts. Teachers of the sciences will always be in full demand in graduate and professional schools and, accordingly, they have no fear of unemployment. But they have a major contribution to make to the undergraduate liberal arts curriculum, which their concern for the liberal arts will not permit them to overlook. The contribution that the sciences have made during the past three centuries to the current pattern of human habits of thought is such as to entitle them to more, rather than less, prominence in the liberal arts.

The typical apologist for the humanities starts with the axiom that the liberal arts is the domain in which his subject is pre-eminent. He then identifies the sciences with gadgetry or at best technology and draws the conclusion that they have no proper place in the higher realm of the spirit in which the humanities dwell. The fact is, of course, that

* The substance of a paper read before the Science Division, Oberlin College.

the sciences are *potentially* (mark that word, for I shall return to it) at least as rich a source of meaningful imponderables as are the humanities. While the practical aspects of the sciences are not to be despised, their significance lies not so much in the multiplicity of inventions, which makes the world so different and life so much safer and easier than it was a century ago, as in the subtle conception which gave birth to these gadgets and which is vastly encouraged by their use—man's confidence in his intellectual supremacy over nature. And what could be more profoundly a matter of the spirit than that?

When science is viewed in this larger aspect, it is not its *effects,* profound and far-reaching though they are, that should be the primary interest of student and teacher alike, but the nature of the instrument itself. It is utterly unique. Literature and the arts have been produced principally by special geniuses, and the rate of such production appears neither to grow nor to improve with passing time. Current masterpieces of art and literature are of no greater merit, nor are they being produced any more profusely today in proportion to the population, than the corresponding products of two thousand years ago. In the sciences, on the other hand, is to be found the first large body of knowledge that is both sequential and cumulative. As a unified army, organized for a sustained assault upon the citadel of human ignorance, there has been nothing to compare with the sciences in the whole recorded development of human thought. It is possible to question the value of the material which the sciences discover; much of it seems trivial to the lay mind. One may also be fearful of the ultimate effect of scientific philosophy on human welfare; many thoughtful men hold science responsible for some of the major ills of the day. But whether for good or for evil, the fact that science dominates modern thought cannot be disregarded.

Teachers of science are not doing as much as they can to cultivate this rich heritage. This is the more regrettable in that relatively little additional time is required, very little in proportion to the values derived. It is rather a matter of the mode of presentation of the subject than of added time for unrelated material. The great bulk of the material is already before our classes in the usual course of conventional science instruction. It is indeed more completely in hand than it can ever be in courses in history of science or philosophy of science. All that is required to bring out the significance of the discoveries being described is the deft manipulation of relative emphases which any skillful teacher uses, consciously or unconsciously, as part of his stock in trade. No science teacher who has a vision of the prominent part played by his subject in the general intellectual enterprise need make any apology to the humanities for the breadth that science can impart to education. But the number who have that vision is disconcertingly small, and still smaller is the number of those who are willing to go out of their way to acquire it.

The value which the sciences might thus contribute to the liberal arts curriculum is, as I have intimated above, largely potential. There is, to be sure, no lack of those who give lip service to such a program. It is when we examine the texts that are used, the actual proportion of the students' study time that is assigned to the pursuit of these aspects of science, and the amount of attention given to them in examinations that one realizes in what low regard they are actually held by the typical science teacher. For, make no mistake, the importance that will be attributed to these elements of science by students, and indeed the measure of the teacher's own confidence

in them as elements of science instruction in liberal arts, is the proportion of emphasis that he gives to them through the conventional channels of instruction. He is in fact only self-deceived when he says, "Oh yes! I regard these as very important and I mention them in my lectures," and then leaves them completely out of the actual corpus of his instructional program.

Yet a liberal arts curriculum has a right to demand the more liberal elements as a prominent feature of science instruction. In their absence there is nothing in science offerings to distinguish between liberal arts science and technical or medical school science. It is precisely at this point that the most damaging criticisms of the sciences could be made by devotees of the humanities if they only knew enough science to make them. H. D. Gideonse, President of Brooklyn College, has remarked:

> Science as usually taught to liberal arts students emphasizes results rather than methods, and tries to teach techniques rather than to give insight into and understanding of the scientific habit of thought. What is needed is a truly humanistic teaching of science.

President Emeritus Nielson, of Smith College, once wrote:

> Especially in the natural sciences is it the case that the temptation to early and intense specialization has produced a specialist capable of training other specialists, but ill-adapted to educating youth between seventeen and twenty-two.

Mark Van Doren has recently written:

> It will be a long time before teachers have the bravery to extend their knowledge beyond the specialties they started with. A truly coherent curriculum demands that they should, and in some millennium they may.

I hope that Van Doren is as mistaken in this outlook as I believe him to be in some of the rest of his educational philosophy, but he had good ground for his pessimism.

Against the background of the inadequacies of their own training, considering the almost complete blind spot which the great majority of science teachers have at this point, it will be a long hard road for them to travel before they are able to meet their responsibilities as real educators in liberal arts colleges. Yet unless they discharge that responsibility, the sciences will, in the course of a century or less, disappear as a significant feature of the liberal arts program. It is cold comfort to realize that with such disappearance will occur the eclipse of the liberal arts college, for it cannot remain liberal after the amputation of what is potentially its most important member.

But if such a catastrophe comes about, science teachers will bear an important share of the responsibility as the body which determined the character of science instruction in liberal arts colleges. The great danger in their teaching lies, as I have already hinted, in their preoccupation with the training of specialists. Not that they should *not* prepare specialists! They will always have to do that and do it well. The danger lies not in doing that job too well but in doing it exclusively. Professor Sigerist, of Johns Hopkins, remarked shortly before the war:

> If the German academic world surrendered so readily to reactionary forces, it was largely due to the fact that it consisted of men who were specialists and nothing else. If we wish to educate a citizen to be able to think in terms of science and a scientist prepared to participate in social action, we must change our teaching.

There is reason to fear that we ourselves are not immune to the malady that overtook the German academic world. Because of the narrowness of specialization characterizing our graduate training and because of the pressure in later professional experience toward developing a fertile and rapidly expanding field, our science teachers have all become primarily subject-matter specialists, and only secondarily educators. In some cases their preoccupation has been with

research; in others with the training of specialists, a very different undertaking than the problem of fitting one's subject into a matrix of general education. Many of them seem, in fact, not to sense at all the change in the teaching problem which has been brought about by the great mass movement toward higher education that has occurred in this country during the past fifty years, still less the tremendous sharpening of that problem that has occurred with the cessation of hostilities. They will have to learn it, as the price of survival of their subjects in the liberal arts program.

The very mold in which all modern thinking is cast was shaped in large measure by such men as Copernicus, Galileo, and Vesalius in the sixteenth century, and the oftener and the more emphatically we say it and tell why, the better. Prior to their time all technological advances were thought of in terms of necromancy—black magic. These men, together with their contemporaries and successors, re-established the faith which had died with the Ionian Greeks, faith that the world was knowable to any man of intelligence, not merely the world of art and literature but the world of nature as well. That conviction had been lost for a thousand years, an era which we now call "the dark ages." When it was regained, a world that had been palsied for a millenium began once more to move forward with the might and majesty that only a deep and abiding faith in its own potentialities could have generated.

Once again, in this twentieth century era of specialization, we have all but lost the conviction that the world is knowable. If the second world war, with its vast acceleration of technology, brings back the prevalent feeling of the dark ages that all technology is necromancy, and encourages an attitude of frustration on the part of the educational world toward the sciences, then the liberal educational program is doomed, and for that doom teachers of science will bear a major responsibility. The fact is that, viewed in the large, scientists have done a very poor job at interpretation of their own field to their friends outside of it. To be sure, the warfare at the research frontier is exacting, and it is natural that those engaged in it should be loth to turn away from it to explain the fine points of the campaign to the people back home. But such preoccupation is suicidal.

The worlds of art, music, and literature have learned that lesson, perhaps because they had a head start of two thousand years on the sciences. Efforts to interpret art, music, and literature to the common man have, at least in recent centuries, been a major concern to men in those fields, not an incidental, perfunctory, and largely unwelcome activity as is the case with the sciences. Interpretation of the sciences is a major responsibility of the science teachers themselves. They cannot delegate it to philosophers or historians, by very reason of the effects of curricular specialization from which the educational world is now suffering.

The opinion is frequently expressed that the wartime demand for the sciences will result in boosting their prestige enormously in the educational scheme. There is reason to fear, on the contrary, that unless wartime acceleration of the applied sciences is followed by a corresponding acceleration in general comprehension and appreciation of the *spirit* of science, a serious rift will have been introduced into the structure of higher education which in the long run may cripple or even destroy it. The greatest and almost the only hope of salvation lies in the colleges of liberal arts. There is no undertaking to which such colleges could more appropriately or effectively address themselves.

RIVER COME CLOSER TO MY DOOR!

By CHARLES MORROW WILSON

IN TROPICAL, little-publicized Honduras the long incorrigible Ulua River is being made to build from silt wastes thousands of acres of the richest of farming lands. In this remote and least-developed of American nations, engineers and farmers from several of the Americas are causing one of the most violently destructive of rivers to change big areas of festering jungle swamps into highly productive farmsites.

The Honduran venture, already well begun, is both a new page in inter-American history and an unusually timely answer to the enormously urgent challenge of soil saving. Further, it is providing a new and timely approach to the age-old problem of man versus the river.

For rivers are far and away the most formidable looters and destroyers of productive lands. By primer economics for the great majority of nations productive soil is the ultimate savings bank and reservoir of survival. Yet fertile soil is what we, like most other nations of the earth, keep right on losing by the billions of tons, and the millions of acre feet. Soils are gouged away by ice and blown away by winds. But the really appalling and decisive losses are by way of the streams and rivers which each year in the United States alone carry away enough topsoil to feed at least ten million people—if the precious stuff only could be anchored and used.

Year after year these stupendous soil losses go on—spreading like some nightmarish and wasting disease over continents and islands, involving tonnage losses which nobody can accurately compute—nor define in credible arithmetic. But one fact is certain: The displacement of soils by surface drainage waters and the misplacement of these soils by rivers is one subject about which exaggeration is next to impossible. For example, experiments made by the University of Missouri in collaboration with the United States Department of Agriculture, with a medium clay loam soil on a slight (3.6 degree) slope, located several miles from a major river channel, showed the following soil losses from water erosion:

With a yearly rainfall of about 38 inches, one acre of the test land, when plowed and left bare, loses about 40 tons of topsoil; when planted in corn it loses about 20 tons; planted in wheat, about 10 tons; and in a sequence of corn, alfalfa, and wheat, about 2.7 tons. If one begins multiplying such typical soil losses by the hundreds of millions of acres in cultivation, he stands a good chance of stalling his adding machine and wrecking his confidence in man's prospect of survival. The experimental findings in soil losses via surface shed overstate those of level field lands, but definitely understate those of tens of millions of areas of sloping cultivations.

There is nothing remarkable about the fact that people generally resent and fear rivers and regard them as chronic liabilities, despite all the pretty songs, nostalgic poems, and cozy local color books about rivers; or the common knowledge that rivers are important, sometimes decisive, to national economies and histories. Yet even in these United States, where rivers have done so much to open frontiers, foal industries, and provide basic transportation, we, a nation of inveterate builders and unrivaled engineers, keep right on spending billions of man-hours and dollars in working against rivers—not with them. Our citizens or government agencies have succeeded in "harnessing" rivers for generating electric power, irrigating dry

lands, providing city water supplies and, in the illustrious instance of the Tennessee Valley Authority, effecting a really farsighted and well-rounded reclamation program; nevertheless, for every dollar or work-hour that we spend to cause rivers to produce value, we spend considerably more than another dollar in toilsome and oftentimes futile efforts to prevent destruction by rivers.

In the United States, as in other countries, a great part of these distinctly negative efforts have failed. We have built thousands of costly miles of levees. In some, though not in all instances, this has caused the bed levels of a given river to rise ever higher above the adjoining valley lands. Sometimes the levees tend to restrict the river's channel and thereby deepen it; frequently, they accomplish the opposite. We built the levees higher, and thereby engendered still bigger overflows and ever graver hazards to more millions of acres of essential valley lands.

It is true that some of the newer plans for river valley authorities and some of the recent works in headwater impoundment are far more constructive and ingenious than the old-fashioned "levees of fear and doubts." But by and large our prevailing estimates and strategies of river control are still the defensive sort. We keep on struggling to keep rivers from hurting us instead of devising ways to make them help us.

"Ol' Man River," the standout song from *Showboat*, reflects with folk-ballad honesty the riverbank worker's solemn awe of the mighty Mississippi—which unlike the poor Negro stevedore is not obliged to sweat and strain, loading barges, lifting cotton bales, or serving time in jail. Ol' Man River just keeps rollin' along, carrying uncounted millions of tons of silt in suspension—which spells more and more millions of acres of once-rich lands grown poorer, and more and more ruinous floods for the future. In terms of the valley man's viewpoint, the folk ditty "River Stay Way from My Door," with its somewhat lyrical pronouncement, "I ain't breakin' your heart, so don't start breakin' my heart," is unusually pertinent.

But the ever-troublesome case of man versus the river is not limited to any particular country or continent. It is a difficulty of mankind generally and a particularly serious problem for most of the Americas, especially true of the American tropics. The story of the Ulua of Honduras is broadly apropos of the grim battle because the Ulua is generally typical of many other difficult and temperamental rivers. Long before the coming of white men native Indians had listed it as a "bad river." The remarkable Hernando Cortes, who had a distinctive skill for appraising farmsites and settlement locations as well as harbor sites, even while dubbing the Ulua *Rio Malo*, helped to found a port settlement near the river's mouth and pointed out the extreme richness of the valley's land.

But the early Spanish farming settlements at the Ulua's mouth presently died, and for three more centuries the swamps waited as a farflung reservoir for mosquitoes and pestilence. In Honduras, as in most Latin-American countries, the towns and capitals grew inland among the foothills and mountains. Except for occasional fever-ridden squatters who built *manaca* shacks and tilled scraggly milpas or gardens along the scantily drained fringe of the built-up riverbanks, the lush coastal valleys of the Ulua were shunned and forsaken. The situation was broadly typical of a great many tropical rivers and of silt-bearing rivers generally, which tend to build up by silt deposit during overflow periods the areas immediately adjacent to their banks to a higher elevation than the more distant lands of their flood plains.

This holds for the general topography

of the Ulua Basin which in a straight line extends in a southerly direction from the Caribbean coastline to the *cordilleras* of the interior near the town of Potrerillos. The actual distance is only about 60 miles but in its snakelike course the river meanders through 120 miles. In the course of time and twisting the river has formed an alluvial plain about four miles wide at its upper end where the Ulua, Blanco, and Comayagua rivers converge, broadening to about 15 miles as the river proceeds towards the sea.

The Ulua emerges westerly from the mountains into this extensive valley. The Rio Blanco, which originates in the Lake de Yojoa region, cuts in from the central section, and the Rio Comayagua emerges from the eastern side to form the mighty Ulua proper. The Comayagua River flows through a break in the Sierra del Mico Quemado (Burnt Monkey Mountains) to join the twisting channel of the larger Ulua in its tortuous course through the sodden, swampy valley that spreads to the sea. In nine miles of its course the tributary Comayagua drops about 44 feet from the Santa Rita gap to its junction with the Ulua, which in turn drops about 36 feet more in 23 miles to the somewhat tenable area of the banana operating center of El Progreso, thence by flattening grade of about 80 feet more in 85 miles to the open sea. The Chamelecón River is another silt-carrying tributary from the interior Copán region.

ULUA RIVER NEAR THE TOWN OF PROGRESO

The worrisome habits of the Ulua system are habitual and recurrent. In Honduras, autumn and early winter, from late September to March, are the heavy rain seasons, though minor spring floods are also experienced. October usually finds the Ulua and its tributaries at flood stage. For an average of 30 days each year the river runs channel-full, and as the heavy interior and local

rains continue, the grim brown waters regularly spill over into the broad valley, in their haphazard manner sweeping away or burying in mud all farms, fields, buildings, bridges, and railroads in the course of the overflow, and eventually spilling the floodwaters farther down into a huge crescent of permanent and pestilential swamp lands. This had been going on for centuries, confirming the Ulua's name as a bad or incorrigible river. Its transgressions tended to grow with the passing years.

As with many alluvial streams, including the Mississippi and others in our country, as foot upon foot of silt settled in its channel, the Ulua's bed and banks were built ever higher above the level of its adjoining valley, and the overflowing silt-loaded waters, flowing without control, deposited the silt haphazardly, choking up smaller tributary streams and otherwise forming barriers to the natural drainage of the valley. The swamp areas grew bigger and more menacing.

The over-all prospects for effecting flood control for the Ulua were bad. One could build up embankments, revetments, and levees, the river bed would become higher, the flood would break farther down perhaps, but next year, or sooner, man's puny work would have to be done again. However, in the instance of the Ulua there was only one reason why people wanted to take farmlands in the swath of such a "mean bastard" of a river. Ulua lands are exceptionally good banana lands, well forward among the world's best. The climate, rainfall, and wind shelters are basically right for bananas, which in matters of soil, drainage, and climate are among the "choosiest" of crops. Hondurans and others had learned this even before the turn of the century, while the international banana trade was still in its creeping infancy.

As United States demands for bananas climbed and multiplied, peasants, local planters, and banana companies alike began to venture deeper into the wet, rich valley lands where the most coveted of tropical fruits grows so lushly. With passing years Ulua banana planting became decidedly important to the expanding banana trade.

By 1919, along with a considerable number of citizen planters, two major banana companies were clearing and planting costly and (they hoped) high-yielding and long-lived banana farms in the Ulua valley. On the east bank, or Tela side of the river, were the men and farms of the already strong United Fruit Company, headed by the shrewd and thoughtfully quiet Andrew Woodberry Preston, of Boston. On the west bank, or Cortes side, were the farms and railroads of the Cuyamel Fruit Company, headed by the even more brilliant Samuel Zemurray, previously of Mobile, New Orleans, and Warsaw.

As a matter of reason and expedience, both banana companies relied heavily on their respective engineering departments. More or less coincidentally, two young civil engineers, Patrick H. Myers of Tarheel, N. C., for Cuyamel, and Tom J. Barnett of Eureka Springs, Ark., for United Fruit, began independent studies of the rambling, flood-gutted Ulua.

The initial work was tough, sweaty jungle-fighting; exploring and surveying the mosquito- and alligator-infested swamps, lagoons, and arroyos; clearing the mighty forest trees and jungle bush, planting the bits or rhizomes of the bananas; laying first railroads, building field hospitals, commissaries, and quarters; and otherwise making the wet, hot valley lands livable and tenable. The banana-growing pioneers, like their predecessors, promptly learned to respect the Ulua's moods and might. Seeking the usual goals of protection from destroying floods and providing essential

drainage of the farmsites, they first raised levees and by use of large dragline excavators discharge channels were opened through the series of clogged and congested swamplands to carry standing waters away from the planted fields and to reclaim for planting thousands of acres of silted lands adjacent to the riverbanks.

It was exhausting work. On both sides of the river big planters and little planters alike continued to view the Ulua apprehensively. At first only fringes of riverbanks and minor areas of the valley lands could be planted. Many of the earlier banana fields were mere strips of two or three rows of the giant, pink-blossomed banana plants, closely paralleling the banks, which provided natural drainage for slim strips of lands immediately adjoining.

Through urgent necessity the banana engineers and farmers began a closer study of the Ulua. Pat Myers and his assistants identified the fact that when the Ulua is at high mark, or "banks full," the river carries approximately one thirty-second of an inch of silt per square foot of land surface per cubic foot of water.

They approximated the Ulua's average annual discharge of water as some 550 billion cubic feet per year. About one-third of the total volume passes through the channel during the principal month of floods. They estimated that as a year-around average the Ulua carries about 137 parts of silt per 100,000 of water by weight. All this suggested that during an average year the Ulua's waters carry in suspension enough highly fertile soil to cover about 25,000 acres, or about 40 square miles, of valley land with a nine- to twelve-inch layer of the rich new soil—provided the silt loads could be dropped at the providential time and place.

Therein lay the real rub. Preliminary analyses of silt contents of the river water, both as to soil chemistry and physical properties, were encouraging. But how could the rapacious river be made to part with the valuable soil? The textbook answer is: "by controlling the volume and velocity of the water flow." That defines the basically simple, highly constructive tactics of "practical sedimentation." In view of the indisputable fact that soil is the most vital of natural resources, why has ingenious man paid so little heed to so vital a salvage?

There is no satisfactory answer to this question. There are a few precedents, notably the century-old silt recovery program as practiced along the Nile Basin, in some instances at distances as great as 50 miles from the actual river channel. But rivers, like people, are never precisely alike. The Nile is one of the longest of rivers. The Ulua is comparatively short. The lower Nile is excessively sluggish. Its grade, or "longitudinal slope," is only one foot to 91,000; the Ulua's is one foot to 4,500. The Nile's silt load is proportionately slight. It deposits silt at the rate of about four inches in 100 years, or around one thirty-second of a foot per year. The Ulua is probably capable of building land at the rate of a foot per year—1,020 cubic feet of the Ulua's waters, containing one and one-third ounces of silt per cubic foot are theoretically capable of placing one foot of silt. But during much of the year the silt loads are greater than one and one-third ounces per cubic foot of water.

Along the Nile the beneficial strategy of soil creation via sedimentation emerged as a by-product of the necessary and vital irrigation practiced in that area. For centuries Egyptian farmers have drained or drawn the Nile's water into settling basins for eventual distribution to valley fields or garden plots. They permit the river silt to settle to the basin bed, built parallel to land contours. After the bottoms of the basin

have become spread with a replenishment of rich soils, they plant the earlier basin sites to crops.

But irrigation was and remains the real motive back of soil building along the Nile. Essential flood controls and farm drainage paved the way to land building along the Ulua. But as an engineering feat, soil building by way of sedimentation along the Ulua is far more difficult than silt recovery along the Nile.

Late in 1919, Pat Myers, the gracious and imaginative civil engineer from Carolina, abetted and supported by Cuyamel's president, Samuel Zemurray, who knows the Ulua Valley as thoroughly as any man living, began a first soil-building venture on the left or west bank of the Chamelecón, shortly above its confluence with the Ulua. The primary objective was that of protecting low-lying farms from violence by floods. The first trial sought only to recover a small fraction of the silt loads available. Nevertheless, within twelve years a first 5,000 acres of land, with silt deposits ranging from six inches to ten feet, had been built up by planned and controlled flooding and silting operations. By moderate estimate, these first river-built lands are worth at least $200 to $400 per acre, or a million to two million dollars.

Still more valuable were the practical lessons learned, and the work manuals provided. In the beginning Pat Myers and his helpers had determined that a current velocity of around 4.5 miles per hour is best for purposes of land building. Faster currents are almost certain to spread heavy gravel, stone, driftwood, and other undesirable materials over the prospective fields without depositing the finer and lighter silt.

The successful exploitation of current

DIVERSION CANAL TO DRAIN A SWAMP AREA IN THE ULUA VALLEY

ULUA RIVER AND CULTIVATIONS DEVELOPED BY SYSTEMATIC SILTING

velocity calls for extreme finesse. Pat Myers and his men began to experiment with spillways at points along the river-bank to encourage the inflow of the flood-waters, and the use of various types of collapsible check dams, or wide-crested weirs, the latter built to facilitate the spreading of silt by waters drawn directly from the river channel and guided by inlet channels leading from the spillways to the lower waste areas. They employed dragline dredges and steam shovels to dig surface canals, or *bouqueras*, by means of which the river waters that poured through the spillways during high river stages could be swept back and forth at controlled velocity to the low stretches of valley where channel grade reduction and the lush swamp growths retarded the current, to cause the silt deposition where desired. That permitted the silt to settle on, and so build up, the swamp sinks that particularly needed building up.

The earlier phases abounded in toilsome and expensive trials and errors. But the rudiments were encouragingly simple: to minimize levee building; to cut into the river channel and take out the muddy water before it reaches bank-full flood stage by diverting it into *boqueras* so built and arranged that the silt-bearing water can be distributed or "swish-swashed" in a manner to retard its current and cause the silt load to be dropped as advantageously as possible; then to provide exits for the silt-free waters.

The method in principle also controls the flood stages of the river and thereby reduces or avoids the former flood damage to improved areas of the valley lands.

Cuyamel's first successful silting projects involved changing 5,000 acres of low,

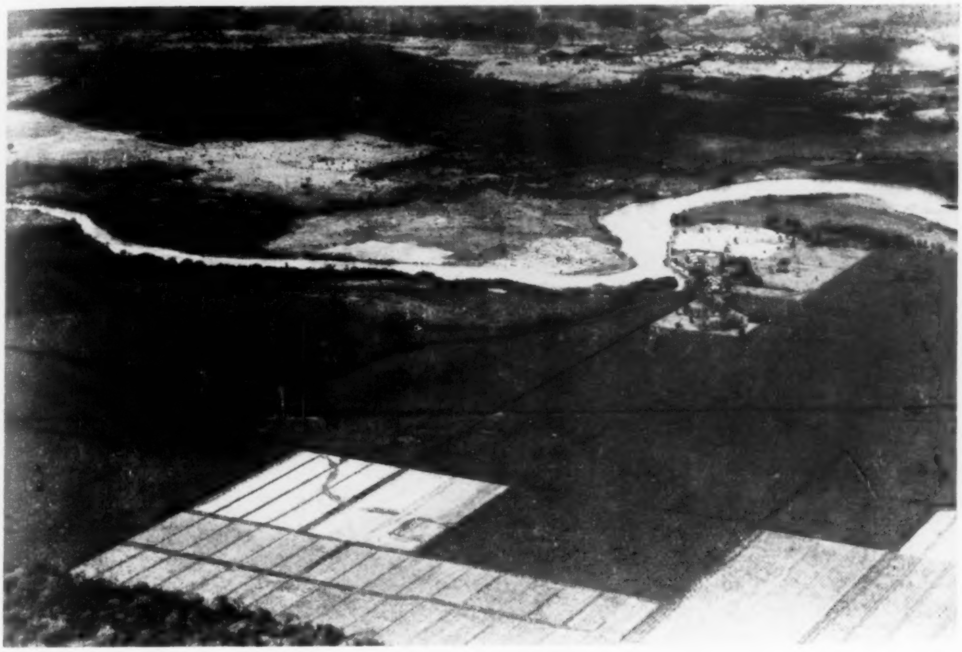
miry valley swamps to tenable lands by the processes and strategies of silt recovery. A typical recovery district included 19 surface channels, each an average of two yards wide and from 2 to 2.3 miles long, spaced about 400 yards apart. To fill these canals silt-bearing waters were taken out of the river channel by way of a broad, shallow intake canal at the riverbank, the canal about 240 feet across and eight feet deep. For discharging the spent waters from the *boqueras* Pat Myers and Cuyamel designed two broad, shallow drainage canals, one about ten miles long, the other about seven.

The first planning, though far from perfect, presently began to "take." In time the *bouqueras* and the swamplands between them became leveled and enriched by silt deposits ranging in depth from six inches to ten or twelve feet. By 1934 several thousand acres of the "made lands" were bearing profitable crops of bananas. Valuable lands had actually been created by controlled sedimentation.

Without any widespread notice the land building continued. Along the east side of the Ulua, the United Fruit men were finding more thousands of acres of bothersome swamps which by drainage and sedimentation could eventually be changed to valuable farming sites. Though the estimates have varied, Barnett and Myers and their assistants confidently predicted that in the valley as a whole at least 90,000 acres, or perhaps 100,000, of useless and troublesome swamplands could eventually be built into superb farming lands by the processes of sedimentation. By late 1929 when Cuyamel and United Fruit joined forces and properties, the land-building technique was rather clearly proved, even though some of its tactical details had not been perfected or agreed on.

But the banana *hombres* continued the land building along the Ulua. They have effected about 9,000 more acres of "upbuilding" on the east bank—in the Palomas and La Fragua sections, on the Tuaymas, Mezapa, Toloa, Melcher and Meroa banana farms, and other banana farmsites, including Guanacastal, Ticamaya, Kele Kele, Tibombo and Manacolito on former Cuyamel lands to the

BUILT LANDS AT THE CONFLUENCE OF THE ULUA AND THE COMAYAGUA



BUILT LANDS ALONG THE ULUA WITH GUANCHIA FARMS IN FOREGROUND

west. More recently privately owned banana properties, such as Finca Oro and the Birichiche Estate, have joined in the land-building venture.

The total was well past 15,000 acres as World War II began, enough to place the work clearly beyond the experimental stages. War shortages of the needed heavy machinery and available ships delayed the work considerably. But the Ulua land-building enterprise goes on—with the strong likelihood that the goal of 90,000 to 100,000 additional acres of river-built farm lands will ultimately be reached.

For the relatively little Ulua that is a big order. It requires, among other things, that the deep lower swamps be striated with immense discharge canals leading 25 miles or more indirectly into the Caribbean. This calls for the excavation of around 75 miles of such canals and the removal of at least 60 million cubic yards of mud, which in turn requires the use of enormously expensive floating and other equipment. The costs are certain to be heavy, at least $100 per "built acre," most probably even more. The background work alone will cost millions. The time factor is likewise considerable; an 18-inch silt deposit in five years is the mean rate of land building contemplated.

Good civil engineering has been and will continue to be essential, a caliber of engineering that can coordinate the necessary flood control and surface drainage work with effective land building. Mechanical engineering is also requisite since the work requires ingenious controls, such as automatic floodgates, to keep the flood loads within river channels and to avoid spreading the heavier sands and debris upon the tillable lands. The task requires skillfully built intake canals; the building of costly cement tunnels to carry silt-laden waters to the more distant field sites; and stubborn succes-

sions of well-designed spillway channels, or sluice cuts, to be filled with silt, blocked off, and replaced with other *bouqueras*.

Drainage of the built lands, which is essential for growing bananas, or almost any other profitable crop, poses further problems. Where "natural" or gravity drainage is not possible the standing water must be pumped away. And there are few absolute rules for building lands from river-borne silt. Each river, indeed each section of river with its immediately adjoining lands, presents specific problems and challenges. In some instances the native vegetation—trees or bushes or grass—can be used advantageously as anchorage for the silt deposits. But in one way or another the river water must be taken out of its regular channel and guided at prescribed velocity to low areas.

Pat Myers, now returned and retired to his North Carolina farm, agrees that building land by sedimentation is not magic—"It only looks and acts like it." Like other apparent feats of magic, inveigling a scoundrel of a river into building valuable land calls for exact timing. The general planning has to be shaped months or years in advance of the location work. But the latter frequently defies rules and calls for improvisation.

The Ulua adventures have proved that valid soil recovery and soil building can be accomplished. The land so built is not necessarily cheap, but it is immensely fertile. With its generally favorable topography and climate, the progress in river building of lands can assure the otherwise untenable Ulua Basin a permanent place as the No. 1 reservoir of the international banana supply, a fact of more than casual importance to consumer markets throughout North America, the British Isles, and Western Europe, with their more or less habitual banana eaters. Only an infinitesimal percentage of tropical lands, probably no more than one-twentieth of one percent, is suited to growing bananas profitably.

As already noted, the Ulua Valley, as refurbished by sedimentation, is probably the world's most favored banana site. But the possibilities of the Ulua land building outreach and outweigh the perpetuation or the well-being of any one crop. Most other crops also thrive on the silt-built lands—more lushly than on any other. Banana growers, in the line of business and with a routine profit motive, have made the Ulua Basin an international proving ground for land building via recovery of river silt.

Though it provides no absolute formula for land building in other river basins, the Ulua venture is significant laboratory work. A great deal of detailed study is requisite to venture a guess as to how applicable its reclamation may be to silt-bearing rivers in the United States or other Americas. But the challenge is defined rather clearly and the scope of importance keeps right on growing. Here in an American background, people are reclaiming at least a fragment of the colossal, incessant soil loot that is collected and in most instances destroyed by rivers.

FROM DUNGEON TO TOWER

By F. L. CAMPBELL

THE continuing appeal of the A.A.A.S. to its members and friends to provide funds for a building to house its administrative, business, editorial, and service personnel has stressed the inadequacy of space that the Smithsonian Institution is now able to provide. Indeed, the activities of the Smithsonian could easily absorb the space that we now occupy, and it is possible that we, as guests, have become a burden to our generous hosts.

Few members of the Association have seen our quarters in the Smithsonian Institution Building. Therefore, we thought it might be appropriate and helpful to demonstrate by means of four photographs the conditions under which we are now working and the use that is made of present space. We are now occupying one room on the second floor (22 × 25 feet), three rooms on the third floor (14 × 14, 17 × 18, and 14 × 14 feet), one room on the eighth floor of the Tower (9 × 13 feet), and one room on the ninth floor of the Tower (9 × 13 feet).

Let us begin at the bottom and work up. To reach the second-floor room, formerly used for storage only, one must go to the third floor, walk to the rear of the building through the herbarium, and descend a stone stairway only 35 inches wide. One feels that he is going down to a dungeon, and so we have come to speak of this room as "the dungeon." Before fluorescent lights were installed the room itself was reminiscent of a dungeon because only a little light enters through the high portholes shown in Figure 1. During the past year it became necessary to use this room for an office as well as for storage. Supplies, symposium volumes, back issues of the SM, and various records were stacked on shelves to the ceiling on three sides of the room. Office furniture had to be taken apart to carry it down the narrow

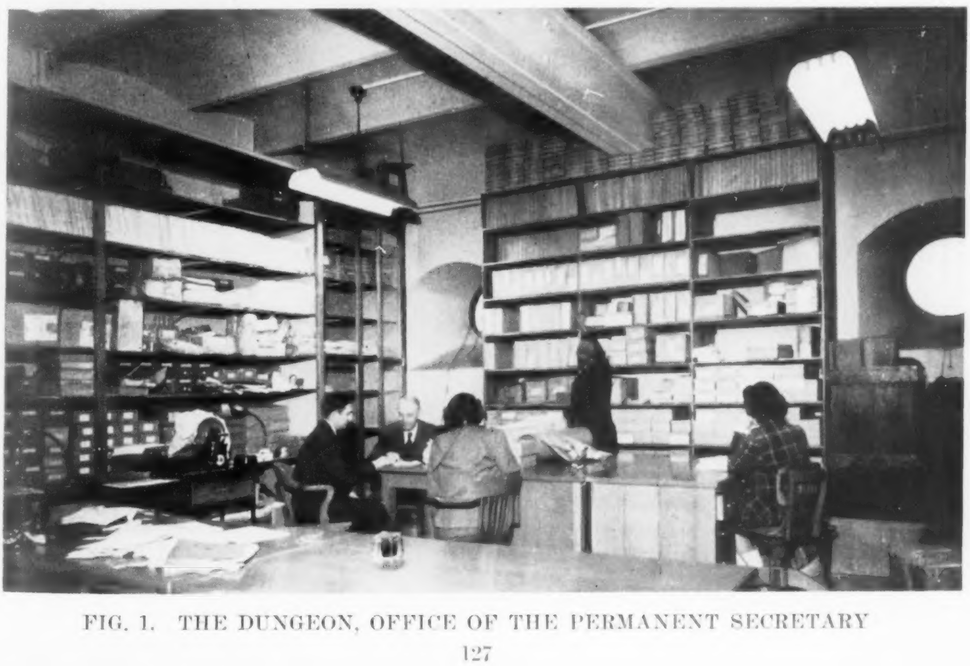


FIG. 1. THE DUNGEON, OFFICE OF THE PERMANENT SECRETARY

stairway. Recently, the Permanent Secretary, Dr. F. R. Moulton, felt it necessary to vacate his office on the third floor and move to the dungeon in order to provide more space for the business office. Thus in Figure 1 we find the chief administrative officer of the Association (facing the camera) talking with a visitor in a room that would be spurned by any executive less determined than Dr. Moulton to serve the Association. We shudder to think of the impression of the Association that visitors receive when they finally reach the dungeon.

We are glad to ascend again to the third floor and go to the front of the building where the Association's business office has fairly normal windows. The photograph shown in Figure 2 was taken from the doorway of Dr. Moulton's old office, now used by the new Administrative Assistant, Dr. John M. Hutzel. Only the central office and a glimpse of the room beyond can be seen in this picture. The filing drawers in the background contain cards bearing the names and addresses of members of the Association and a record of their payment of dues, etc. Desks and other filing cabinets leave little room for passage. Here must be handled thousands of checks, subscriptions to the SM, inquiries regarding membership and journals, changes of addresses, orders for copies of symposium volumes and the SM, solicitation of new members, and—perfection being impossible—complaints. The volume of record-keeping and mailing done in this little office is staggering, and the lights burn there nearly

FIG. 2. RECORD AND ACCOUNTING OFFICE OF THE A.A.A.S.

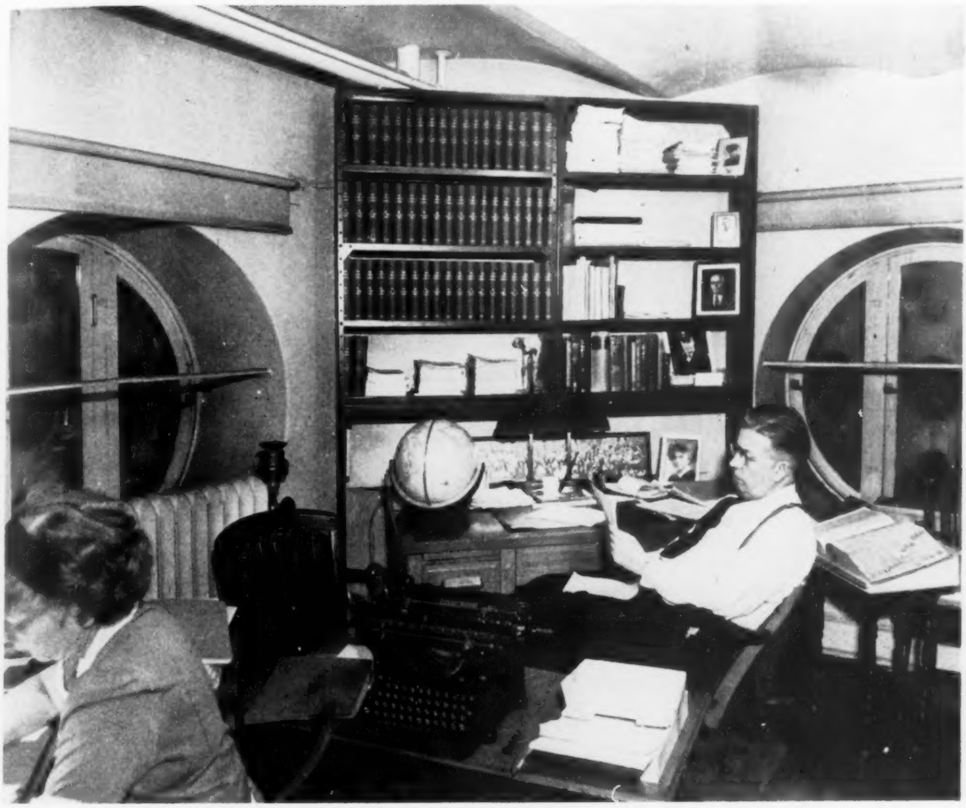


FIG. 3. OFFICE OF THE EDITOR OF THE SCIENTIFIC MONTHLY

every night. In the far room are the files of addressograph plates and the addressograph and stamping machines, together with supplies and a large collection of college and university catalogues. We wonder that the clerical help is willing to work under such pressure and crowding, and, indeed, the turnover has been high. The training of each new employee adds to the almost intolerable burdens of the manager of this office.

Let us leave this sweatshop and go to the best of the Association's rooms—the office of the editor of the SM on the eighth floor of the Brownstone Tower. To be sure, it is very small and a firetrap, but the view is inspiring, and it is warm in winter and cool in summer. The photograph (Fig. 3) shows about half of the room including the north and west windows. The editor has his feet on the visitor's chair, and his assistant works at her desk. We could not show the vertical steel ladder in front of the south window, nor the elevator, sink, and filing cabinet. In case of fire we would have to open the trapdoor in the floor, climb down the ladder to the floor below, and open a window to a fire escape that leads to the roof of the main building. After having reached the roof, we could probably find a spot from which we could jump into the firemen's bouncer. If it is necessary to enter or leave the room when the elevator breaks down, the ladder must be used. Fortunately the elevator is fairly dependable, and I have had to climb the ladder only about twice a year. For a woman, how-

FIG. 4. THE EXECUTIVE SECRETARY'S OFFICE IN THE BROWNSTONE TOWER

ever, the prospect of using the ladder is not pleasant.

The same hazards and inconveniences apply to the floors above. The ninth floor of the Tower (Fig. 4) is occupied by Dr. Howard A. Meyerhoff, Executive Secretary, and Mrs. Meyerhoff. When Dr. Meyerhoff moved into this room, containing only a table and chairs (the cabinets are used by the Smithsonian), it was understood that he might be asked to vacate at any time. He could move into the dungeon, but his makeshift room in the Tower seems the lesser of two evils, so he clings to it. Because his space is temporary, he does not have filing cabinets or a telephone. A geologist, he must use what Austin H. Clark calls the geological filing system, that is, the building up of sedimentary deposits of letters on a table top. If he can remember the various strata on the table, he may find what he wants when he wants it.

Dr. Meyerhoff uses our telephone, and we call him to it many times a day either by shouting or pounding on his trapdoor. Unless the elevator is standing at his floor, he opens the trapdoor and climbs down the ladder to answer the telephone, then he climbs up again. It is very good exercise for an office worker, but somehow it seems ridiculous for an executive officer of the Association to spend so much time climbing like a monkey.

Our medieval excursion from dungeon to tower is over. Tell us what you think of it—preferably by sending us a slip of paper inscribed, "Pay to the order of the A.A.A.S. Building Fund."

AFRICA NEEDS PALMS AS TREE CROPS

By O. F. COOK

The native flora of tropical Africa is remarkable for the paucity of palms. Each of the continents has its distinctive palms, but the African series is extremely short. In the absence of undergrowth palms, the African forests appear much less tropical than those of Central and South America, and most of the open bush country in Africa is without any palms. African coast settlements have a few coconut, date, or oil palms planted in gardens or allowed to grow from chance seedlings, and the oil palms often spread in wastelands, but relatively few interior districts have palms in abundance. Thousands of photographs of interior landscapes have been published in works of travel during the past half century, but they seldom show palms, and in these books the native kinds are rarely mentioned.

Although the tropical area of Africa is the greatest of any continent, most of it is wasteland, denuded and depopulated through recurring cycles of native agriculture and the repeated cutting and burning of the forest to make temporary clearings for rice or other field crops. The tillage methods often urged by visitors from temperate regions are destructive, inducing more rapid erosion. The practical alternative in tropical agriculture is to develop the use of trees and tree crops to obtain permanent protection for the soil. In such a reclamation of Africa by tree crops the palms may be expected to contribute more than any other group of plants. Some of the native palms are capable of being utilized extensively, and many useful kinds from other countries could be introduced. The range of specialized adjustments to particular conditions of growth is remarkably varied among the members of the palm group, from shaded forest depths and seething tropical swamps to chilly mountain summits, windswept seacoasts, and the barest, hottest deserts.

In the tropical belt of Africa only four families are represented among the native palms, and the indigenous genera are much fewer than in Asia or in the islands of the Indian Ocean. Other palm families are highly developed in the Malay region, but are wanting in Africa. All the genera in tropical Africa are the same as or closely related to palms found in Asia or in Madagascar, the island itself, however, having many more palms than the continent. Queensland, the tropical corner of Australia, has more native palms than the entire continent of Africa.

From tropical Africa about fifty species of palms have been named, whereas from the American tropics more than twenty times as many palms are known, and many new species are still being discovered. In contrast to the four families of palms found in Africa, fourteen families are recognized in America, most of them not represented in the Old World. Cuba, Haiti, and Trinidad have notably richer palm floras than tropical Africa. Even the tropical extremity of Florida has its native palm flora with nearly as many kinds as the tropical belt of Africa. The palmettos frequently appear as dominant features of the landscape in Florida but never in Africa. Two of the African genera, Chamaerops in North Africa and Jubaeopsis in South Africa, are not found in the equatorial belt, but are definitely extratropical, like the palmettos of our southeastern states and the molasses palm of Chile, *Jubaea chilensis.* The Chilean and the South African palms are closely related, as are the

Mediterranean Chamaerops and the Serenoa, or saw palmetto, of Florida.

On the American continent the tropical vegetation includes diversified local floras of small undergrowth palms, often a dozen or twenty kinds in the same locality. There are hundreds of species of these small shade-tolerant palms in tropical America, classified in numerous genera of several families, with various adaptive features. Other groups of plants in tropical America are remarkably specialized for life in forests as undergrowth or as epiphytes, notably cyclanths, orchids, begonias, bromeliads, cannas, and Heliconias. The Asiatic series of specialized shade plants also includes many palms not represented in Africa.

The comparative development of the undergrowth types of vegetation may be taken to indicate the relative age of forest associations of the continental areas. In such a reckoning the forests of tropical America appear to be the oldest, those of Asia intermediate, and the forests of Africa more recent, a relation consistent in general with the westerly direction of the trade winds. The world-wide distribution of the ancestral palms doubtless was attained in advance of the forest period. Continuance of the open preforest conditions in Africa is indicated by the wealth of terrestrial mammals, whereas in the forests of South America the mammals are largely arboreal, hardly less specialized than the birds for living in the treetops, as Bates so vividly described in *The Naturalist on the River Amazons.*

Rattans the Only Forest Palms. In most of the African forests the only palms, if any, are the very slender, long-jointed rattans trailing like lianas among the treetops, where travelers seldom see them. The leaves of the rattans have long midribs extended far beyond the pinnae to form slender climbing organs, beset with hooked spines. Botanists have recognized Calamus and four other genera of rattans in Africa, but with differences apparently no greater than among the hundreds of Malayan species referred to Calamus or to Plectocomia, all with similar habits of climbing above the forest canopy and keeping out of sight, unless from airplanes.

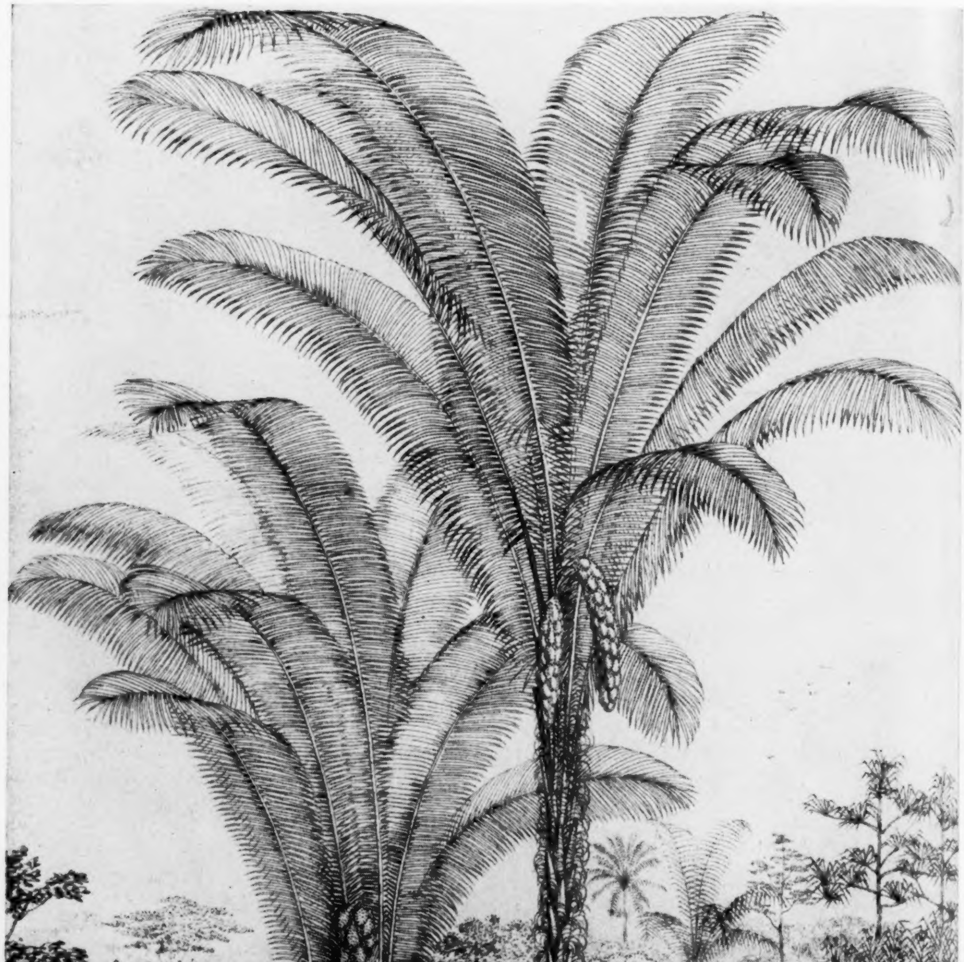
Two localized genera of very small palms, Sclerosperma and Podococcus, were described in 1864 by Mann and Wendland from swampy places along the Gabon and neighboring rivers, but apparently not spreading as forest undergrowth. Excellent drawings of these palms and of two of the African rattans were published and are reproduced in Figure 1. Young rattan palms, before the stage of climbing, are shown at the lower right-hand corner of the illustration. Other localized species of Sclerosperma and Podococcus have been reported from neighboring districts, but the later explorers have not discovered additional genera.

The Raphia, or Wine Palms. After taking account of the rattans and the oil palms only four other African palm types remain. The type that occurs most widely, scattered along riverbanks or clustered in open swampy lands, is known in Africa as the wine palm, the genus Raphia, and is found also in Madagascar and in tropical America. The trunks usually are short and the leaves few, very long, and spreading. The African wine palms formerly were supposed to represent only one species, *Raphia vinifera*, but several local kinds have been named. Two were described by Mann and Wendland from the island of Corisco off the coast of Cameroon: *Raphia longiflora* and *Raphia hookeri*, the first with short trunks, the second a rather majestic palm, with trunks thirty feet high and leaves forty feet long (Fig. 2). The larger species was reported to have been cultivated by the natives at old Calabar.

From Mann and Wendland

FIG. 1. FOUR SPECIES OF AFRICAN PALMS

ON THE LEFT ARE TWO DWARF PALMS, *Podococcus,* WITH SEPARATE SLENDER TRUNKS, AND IN THE CENTER IS *Sclerosperma,* WITH THE LEAFSTEMS CLOSELY CROWDED. IN CENTRAL BACKGROUND AND ON THE RIGHT ARE TWO CLIMBING HOOK-LEAVED RATTAN PALMS: *Calamus laevis* (CENTER) WITH RELATIVELY SHORT LEAVES AND TAPERING PINNAE, AND FIVE STALKS OF *Calamus secundiflorus* (RIGHT), THE THREE UPPER STALKS WITH LEAVES OF THE MATURE FORM AND TWO YOUNG STALKS BELOW.



From Mann and Wendland

FIG. 2. TWO AFRICAN WINE PALMS

THE WINE PALMS ARE CHARACTERIZED BY THE VERY LARGE LEAVES WITH REGULAR SPREADING PINNAE. TWO SPECIES OF WINE PALMS WERE FOUND BY MANN AND WENDLAND GROWING ON THE ISLAND OF CORISCO IN THE GULF OF GUINEA. THE SMALLER SPECIES, *Raphia longiflora*, HAS ONLY A SHORT TRUNK LIKE MOST OF THE OTHERS, BUT *Raphia hookeri* IS REMARKABLE FOR ITS SIZE—THE TRUNK MAY BE 70 FEET HIGH AND THE LEAVES 40 FEET LONG WITH 190 TO 200 PINNAE ON EACH SIDE OF THE MIDRIB.

The fruits of Raphia are characteristic, two to four inches long, elliptic in outline, and covered with smooth, shining scales like pine cones. Each fruit contains a single, large ivory-hard seed, the surface coarsely wrinkled and surrounded in the fresh state by a thin flesh, yielding an edible yellow oil like that of the oil palm, Elaeis. Fibers of two kinds are obtained from Raphia: a soft so-called raffia stripped from the epidermis of the young unopened leaves, and a coarse piassava fiber retted from the leafstalks.

The Borassus, or Palmyra Palm. The massive palmyra palm, *Borassus flabellifer,* extensively cultivated in the East Indies, is represented in Africa by a closely related species, *Borassus aethiopum,* one difference being that the trunk of the African species is more definitely thickened near the middle. A height of 100 feet or more is attained, with a diameter of two to three feet. Forests of Borassus are reported in the western Sudan and along the coast of Angola, and it occurs locally from Senegal to Mozambique. The stiffly radiating segments give the leaf blades a spread of about five feet. A vigorous young palm growing in the open forms a cylindrical crown of deep green leaves reminiscent of the early stage of *Washingtonia robusta* in California. A young Borassus palm on the St. Johns River in the Bassa district of Liberia is shown in Figure 3.

The fruit of Borassus is nearly round, six to eight inches in diameter, the largest of any palm except the coconut and the so-called double coconut of the Seychelles Islands near Madagascar, which is related to Borassus. The husk of the fruit is smooth outside and fibrous within, normally containing three flattened, fiber-covered, hard-shelled nuts, embedded in a sweetish pulp. In India the palmyra palm is valued chiefly for its sugar-bearing sap, while the trunk, leaf stalks, and leaves are a source of wood, fiber, and paper. The germinating seedlings form a starchy, edible underground bulb like an inverted parsnip. As in most of the palms, the terminal bud is tender and edible, like cabbage, though in a few kinds the tissues are too bitter or acrid to be eaten raw.

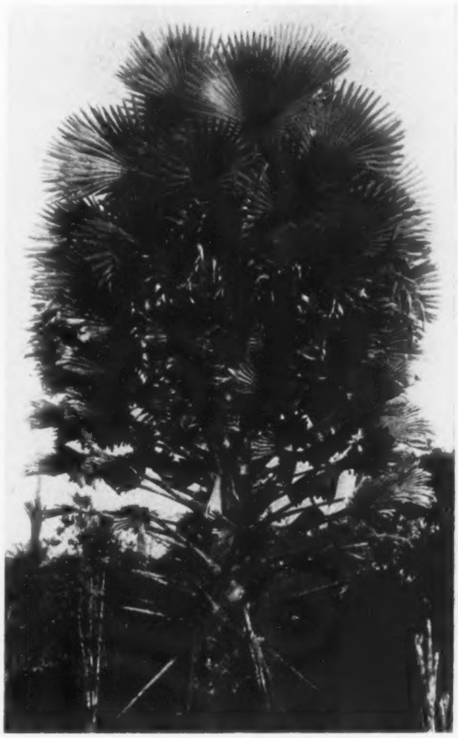


Photo by Guy N. Collins

FIG. 3. A YOUNG BORASSUS PALM

THIS SPECIMEN, NEAR ST. JOHNS RIVER, LIBERIA, IS GROWING IN A NEGLECTED COFFEE PLANTING.

Commercial Importance of the Oil Palm. One palm in tropical Africa has outstanding commercial importance, the African oil palm, *Elaeis guineensis* (Fig. 4), which furnished the principal exports of the West Coast of Africa after the slave trade was suppressed. Because palm oil was used for feeding the Negroes on the slave ships, it was generally believed that the oil palm was indigenous to Africa, but, as I have shown in the SM, June 1942, it now appears to have

been introduced (Fig. 5) from Brazil in the early colonial period to the Portuguese settlements on the West Coast. The oil palm grows in a wild state on the coast of Brazil, and a closely related genus (Alfonsia) occurs throughout northern South America and in Panama and Nicaragua. The oil palm is now widely distributed in tropical Africa, especially along the West Coast, and often in great numbers, since it spreads readily in open places.

The trunk of the oil palm is somewhat thicker and more erect than the coconut palm, with the leaves more numerous and forming a dense, feathery crown, on account of the pinnae standing at various angles to the midrib, and not in regular lines as on coconut leaves. The plum-like fruit grows in densely crowded heads protected by needlelike spines. The yellow palm oil comes from the outer flesh surrounding the black palm nut, with its hard, brittle shell and its white palm kernel, in taste and texture like the meat of the coconut. The kernel oil is clear, transparent, and tasty like coconut oil. In addition to the yellow palm oil, vast quantities of kernels are exported and the oil is expressed in Europe and America. In Africa, for emergency use, a little kernel oil may readily be extracted by parching handfuls of kernels for a few minutes in a hot kettle, for use with rice or cassava.

The Date Palms, Cultivated and Wild. The cultivated date palm, *Phoenix dac-*

Photo by Guy N. Collins

FIG. 4. OIL PALMS IN LIBERIA

THESE PALMS DEVELOP ONLY IN OPEN PLACES. HERE THEY ARE GROWING BY THE SEA NEAR MONROVIA.

From Monteiro

FIG. 5. OIL PALMS ON THE QUANZA RIVER, ANGOLA

IN THIS DISTRICT THE EARLY PORTUGUESE MISSIONARIES MADE PLANTATIONS OF COCONUT AND OIL PALMS. ANGOLA WAS COLONIZED BY WAY OF BRAZIL, WHERE THE OIL PALM OCCURS IN A WILD STATE.

tylifera, is a desert species, planted chiefly in Egypt and the oases of the Sahara, also in Senegal and the Sudan. As recognized in Pickering's *Races of Man* (1876), the cultivated date palm probably was not indigenous to Africa. Bertram Thomas in *Arabia Felix* (1932) shows a photograph of wild date palms growing on sandy flood banks in a previously unexplored district of southeastern Arabia, these conditions affording full exposure, with no competing vegetation. The photograph of these Arabian palms shows large symmetrical crowns of spreading leaves, only remotely like the wild dates in Africa. The cultivated date is definitely a sun palm, not making vigorous growth under shade conditions.

The native dates (Fig. 6) in tropical Africa, usually referred by botanists to *Phoenix reclinata* or to *Phoenix spinosa*, are slender, small-fruited palms, forming clusters of offshoots among stunted vegetation along the seacoasts, and sometimes on riverbanks or in swamps. These wild date palms are little used by the natives, but because they sometimes furnish fiber and palm wine, they are often confused with Raphia palms. A critical distinction is that the pinnae of the date palm leaves are V-shaped in cross-section, because in the bud stage the pinnae are folded upward, whereas Raphia and all the other palms have the pinnae folded downward, with the groove underneath. The date palms in reality are related to Chamaerops and other fan palms but appear different on account of having separate pinnae on a strong midrib. The leaves of fan palms are formed of radiating segments folded upward like the pinnae of Phoenix, but not separated.

The Doum, or Ginger Bread Palms. The doum palm of the Egyptian deserts is *Hyphaene thebaica*, and similar species are widely scattered in the Sudan and other dry districts, south to Mozam-

From Pechuel-Loesche

FIG. 6. TROPICAL NATIVE DATE PALMS

THE NATIVE DATE PALMS OF TROPICAL AFRICA HAVE SLENDER, CURVING TRUNKS AND SEND UP SHOOTS FROM THE GROUND, FORMING LARGE CLUSTERS WHEN GROWING IN OPEN PLACES. UNABLE TO GROW IN FORESTS, THEY ARE GENERALLY CONFINED IN NATURE TO SWAMPS, RIVERBANKS, AND SEACOASTS.

bique and Angola (Fig. 7). The leaves are fan-shaped as in Borassus, but the palms are much smaller. The fruit, only two to three inches in diameter, is somewhat pear-shaped or irregular. It has a hollow kernel, white and very hard, like vegetable ivory, encased in a black, bony shell, and this is surrounded by a rather woody, brownish rind with a pungent, sweetish, "gingerbread" taste and smell, not unlike the edible pods of the mesquite or the carob. A similar palm called Medemia is reported from Nubia as having bitter, inedible fruits. Seeds of Medemia have been found in ancient graves.

Remarkable resistance to heat and dryness is an outstanding feature of the doum palms and may render them useful in the reclamation of districts that now are badly deforested. Even in the superheated depressed valleys explored by Nesbitt between the Abyssinian highlands and the Red Sea, doum palms were found growing in the salt-sinks, beyond the range of any other vegetation and under the most extreme conditions. The title of Nesbitt's book, *The Hell-Hole of Creation,* seems appropriately to mark this outstanding adaptation of the doum palms to difficult conditions. The lack of a proper desert flora of surface succulents or other plants specialized to grow in the open in the Egyptian and Arabian deserts may mean that original forest covering was general and that the present extensive wastes are artificial.

Reclamation with Tree Crops. The need of tree crops is urgent in many districts on account of the rapid increase of native populations during the period of colonial control of tropical Africa by European nations. Many of the primitive tribes have reached the limit of their food supply. The native life may be said to have ended, since a different footing must be found for the future. Cutting and burning the forest to make temporary clearings for rice or other field

crops has resulted in extensive replacement of forests with open wastes or fire-swept grasslands. Tree-crop agriculture is more desirable, because erosion is avoided and permanent systems of production become possible.

Tree crops have been little used in tropical countries, and those contributed by families of plants other than palms are mostly fruits or nuts, such as the avocado, the sapodilla, the mango, the cashew, and the Brazil nut. However, in the palm group alone, five of the principal requirements of human existence—starch, sugar, oil, fiber, and building materials—are obtainable from tree crops. Some of the useful palms, such as the date, the palymra, the sugar palm (Arenga), the coconut, and the peach palm (Guilielma), have been cultivated elsewhere for long periods, and several others have been exploited extensively in the wild state. Examples are the sago palm (Metroxylon) and the rattan (Calamus) in the Malay region; the wax palm (Copernicia), the babassu (Attalea), and the assai (Catis) in Brazil; the vegetable ivory (Phytelephas) in Ecuador; and the molasses palm (Jubaea) in Chile. Other oil palms of potential value have only recently become known, such as Bornoa in Haiti and Ynesa in Ecuador.

Wider utilization of the oil palm in Africa may be a first project in tree crops. Palm oil is produced extensively in West Africa, but usually from wild palms. The seeds are widely scattered, and a little care in keeping the weeds from shading and smothering the volunteer seedlings might increase the food supplies of many tribes in a few years. A basic ecologic fact has been overlooked, even by scientific writers; the oil palm, like the coconut, is definitely a sun plant, requiring full exposure for normal development. Among the natives of Liberia fruits of the oil palm often are carried to eat on the road, the pulp chewed off and the nuts thrown aside, so that native pathways miles away from the palms may be bordered with seedlings, even in forests too dark for any of the seedlings to grow up. An indigenous palm commonly used and scattered by the natives should have spread long ago completely across the continent, instead of being restricted largely to the West Coast. Varieties with thick pulp and thin shells are being cultivated on a commercial scale in the British and French colonies. Oil palm plantations in the East Indies were expected to prove more profitable than coconuts. A general utilization of oil palms may be foreseen in many tropical countries.

From Pechuel-Loesche

FIG. 7. A DOUM PALM

FROM WEST AFRICA, CLOSE TO THE CONGO RIVER.

THE LENGTHENED SHADOW OF A MAN AND HIS WIFE—I

By JAMES G. NEEDHAM
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THIS is the story of the development of a new department of university instruction, the Department of Entomology in Cornell University. In attempting to tell it briefly I will first set down what I have heard from the lips of others, my academic elders, and then something of what I myself have heard and seen.

THE MAN who founded the Department, John Henry Comstock, came to his lifework by a very circuitous route. There was little in his early environment to urge him toward a scientific career. His deep love of nature he seems to have had from his mother. His parents were poor. His father, Ebenezer Comstock, had been a schoolteacher in Massachusetts. His mother, Susan Allen, of the family of the colonial patriot Ethan Allen, was a woman of quality. After their marriage they went west and settled on a small farm at Janesville, Wis. There in 1849 John Henry Comstock was born.

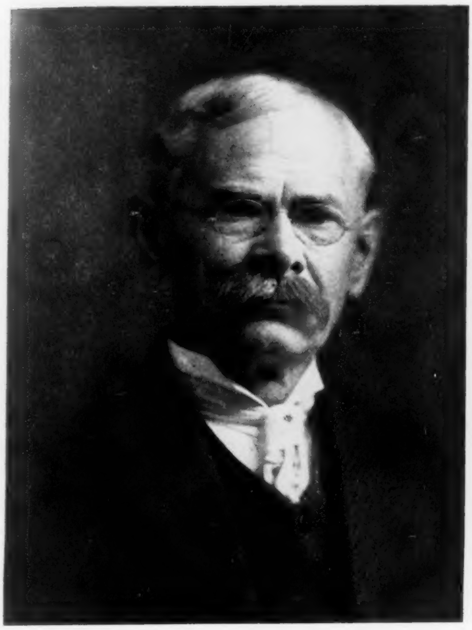
That was the year of the big gold rush to California. The prospects of the farm were poor, and the father thought to better the affairs of his family by joining other adventurers who set out westward so eagerly to find gold. The covered-wagon emigrant train with which he journeyed met with sudden disaster. Cholera broke out, and Ebenezer Comstock was one of the many who died of it. His wife was left to struggle alone with the care of her baby and the farm.

Further misfortunes followed her. The farm was lost on a mortgage. She returned to New York State to be among her own people and found work as a nurse. Her own illness and hospitalization followed, and the boy had to be cared for among several families of relatives until at the age of eleven years he began to make plans for himself.

He chanced to meet Captain Lewis Turner and his good wife, Rebecca, and to win their interest. The captain was a master of schooners that sailed the Great Lakes in the grain and lumber trade. He had four sons who were sailors, already gone from home. The captain offered the boy board and clothing and three months' schooling a year in return for his services about the place, and the offer was accepted gladly.

The captain's wife was an intelligent, kind, motherly woman and an excellent cook. She took the little stranger to her heart; cared for his health, which was none too good; encouraged him to study and to cultivate good habits. She called him "Hanky," and he called her "Ma Becky." Her house became his second real home. He lived there five years, going to school in the short winter sessions and doing all kinds of chores about the place. As long as Ma Becky lived he returned to it at intervals as to a home.

Living among sailor folk, he naturally looked toward sailing as his first remunerative occupation; but he had never been robust and was not strong enough for the heavy work of a sailor on the deck. So Mother Turner, with kind and generous and competent foresight, taught him to cook, and thus equipped him for the place of steward on a sailing vessel. For the next five years he sailed the Great Lakes in summer and attended school in winter. He saved his wages to buy books and to pay tuition in the



Lent by Professor G. W. Herrick

JOHN HENRY COMSTOCK AND HIS WIFE, ANNA BOTSFORD COMSTOCK

private academies that then gave the preparatory work for college, which now the public high schools have largely taken over. On shipboard he always had some book at hand, and he studied it during intervals of work.

It was in reading that he first found opportunity to follow his bent for natural history. He found some books on botany that enabled him to acquaint himself with the flowering plants of the Great Lakes region. Then once, when his ship was at anchor in the port of Buffalo, he went to a bookstore to see if he could find a book that would tell him something about flowerless plants. He found instead another kind of book that opened for him a new door to the house of knowledge. It was *A Treatise on Some of the Insects Injurious to Vegetation*, by Thaddeus William Harris, M.D., Librarian of Harvard University. It was, and is to this day, a beautiful and worth-while book. Its illustrations quickly led him to see that here was something that would interest him beyond anything he had ever read. Its finely colored plates fascinated him; alas, the price ($10.00) seemed entirely beyond his means. He went sadly back to his work without it; but even as he cooked he could not forego the possession of the precious book. He returned next morning and bought it, with money taken from the fund he had been saving for a course in college; and for a time it was his chief possession. That book had a large part in determining his future career. His own well-worn copy is now one of the chief treasures of the Comstock Memorial in the Library of Cornell University. On its flyleaf is written such a tribute as few books can ever claim:

> I purchased this book for $10.00 in Buffalo, New York, on July 2nd, 1870. I think it was the first entomological book I ever saw. Before seeing it I had never given entomology a thought; from the time that I bought it I felt that I should like to make the study of insects my life's work.
>
> J. H. C. Nov. 19th, 1876

At the age of twenty he was ready for

college. He had heard of a new kind of college recently opened at Ithaca, N. Y., where courses of study in science were placed on a par with the classics, and that appealed to him; so he sent for a catalog. From the catalog he learned that the founder had provided means of earning a living while studying, and had said he wished to found an institution where any person might find instruction in any subject; and that all seemed suited to his circumstances and wishes. So he went to Cornell University to study entomology.

When he got to Cornell he found there no course offered in entomology. There was, however, a department of zoology, with Dr. Burt G. Wilder at its head, ready and willing to give him every aid he could to study entomology by himself. Dr. Wilder gave him facilities for study, helped him with books and counsel and encouragement, and after a short tryout of his help at chores about the department, took the youth to be his own personal assistant.

That was a happy association. The eager, industrious, and resourceful boy was exactly the kind of helper that Dr. Wilder needed, and he, the kind of teacher and friend that the boy needed.

Dr. Wilder was a gentleman of the old school, tall, dignified, precise, and exacting almost to a fault, but kindly and generous, and more exacting of himself than he was of others. He was a gifted musician who wrote songs, both words and music, for his own diversion. He was strictly opposed to the use of alcoholic beverages and tobacco; also—alas, for his popularity with the Hurrah Boys of the student body—opposed to fraternities and intercollegiate athletics. He had graduated from the Lawrence Scientific School at Harvard University in that day when the scientific studies that he pursued were kept apart from the classics as representing a lower order of intelligence. During the War between the States he had been, first, a medical cadet, then an assistant surgeon, and then a surgeon in the 55th Massachusetts Infantry. At the time of his appointment to the original faculty at Cornell University he was an assistant in comparative anatomy in the Museum of Comparative Zoology. But he professed no knowledge of entomology beyond the pittance of it to be had in a course in general zoology. That he was not far from being an entomologist himself is shown by what he wrote about himself for publication in the fourth edition of *Who's Who in America:* "In 1863 [he] discovered n[ea]r Charleston, S. C., a large spider (since named Nephila Wilderi by McCook) from which while alive he reeled 150 yards of silk."

He published a number of small papers on spiders; and in the third issue of the *Cornell Register,* a list of donations to the University includes this item: "6. A collection of insects deposited by Professor Wilder."

By the end of his sophomore year Comstock had made such progress with his studies on insects, and such an impression for zeal and competence in all his work, that thirteen of his fellow students petitioned the University faculty to allow him to give them a course of lectures on entomology, with university credit. Dr. Wilder approved the petition and it was granted. The title that Comstock gave his course, "Lectures and Field Work in Entomology," indicated that he meant to make the course practical; that he was not content to have his pupils know insects only as dried specimens stuck on pins. He took his class afield for first-hand knowledge of what insects do; of the roles that they play in the world of life; how they get along together; and how they affect human interests. Many years later he told me that President White had charged him to keep ever in mind the interests of the farmer.

The first course was given during the third term of the following year, his junior year. By that time the second permanent building of the University, McGraw Hall, had been completed, and Comstock was assigned a small room in its square tower as his first entomological laboratory.

The members of that class were Comstock's fellow-undergraduates. Among them were David Starr Jordan, Leland O. Howard, and William Trelease. Jordan wrote of the situation forty years afterward: "I have to remember Comstock as one of the very ablest of my students and one of the most inspiring of my teachers. . . . We stood doubly in the relation of teacher and student. . . . Comstock taught me all that I know and most that I have forgotten on insects, and I taught him the names and habits of the flowers of Western New York. We were boys in those days."

Dr. Wilder taught Comstock academic decorum. He was insistent on gentlemanly conduct, and on the use of good English in written reports. He stressed five characteristics of good scientific writing and called them "The 5 great C's": Correctness, Clearness, Conciseness, Consistency, and Completeness. His oft-reiterated warning to students who were preparing papers for publication was: "Print in haste and repent at leisure."

Comstock's zeal for the study of insects did not kill his interest in other fields. He eagerly attended the University concerts and the lectures by distinguished scholars, whom President White brought to Cornell as members of his "non-resident faculty." These were later called Visiting Professors. Among them were James Russell Lowell, George William Curtis, Bayard Taylor, John Stanton Gould, and others of like prominence in America, and the English historian, Goldwin Smith, who soon became a member of the "resident faculty," and a dear friend of Comstock. Foremost among these distinguished visitors in Comstock's interest was the great Swiss naturalist, Louis Agassiz, with whom Dr. Wilder had been closely associated at Cambridge and at Penikese.

Agassiz gave a course of twelve lectures in 1872 and again in 1873. At Cornell as elsewhere they won instant and enthusiastic interest, and gave an impetus to the first-hand study of nature that has never been wholly lost. His startling advice: "Study nature, not books" (not meant to be taken too literally), was intended to rescue zoology from the bookishness of the schools and to put American students in the way of discovering facts for themselves by the use of their own eyes.

While at Ithaca Agassiz taught by example as well as by precept. He climbed the hills and viewed the smoothly glaciated slopes that drop down to the deep blue lake in the valley. He walked the shores; he waded the streams and searched their shoals for the nests of fishes. Dr. L. O. Howard told me that he once came upon Agassiz, clad in a long black coat and rolled-up trousers, standing in the water in the rocky bed of Cascadilla Creek in downtown Ithaca. He had just lifted a flat stone from the water and held it in his hand. Seeing Howard, he said: "Come here, boy, and let me show you someting eenteresting." It was the roof-stone from over the nest of the little sculpin fish called Miller's Thumb that he held in his hand. Attached to the underside of the stone were the pearly eggs that that little fish in some way manages to hang from the ceiling of its nest.

Dr. Wilder watched Comstock's progress with a fatherly interest; saw that he was going "pretty fast and pretty far" on the basis of very limited experience; saw that he needed contact with the minds of those more experienced and more eminent in his field. And it was doubtless in aid of Comstock's work,

and on Dr. Wilder's recommendation, that Charles Valentine Riley was brought to Cornell for a short course of lectures on entomology.

Riley was then a young man, and already among the best-known of American entomologists. He may have been the only one who was devoting his time wholly to entomology and earning his living by it. There were others, of course, studying insects; several physicians who obtained time for the study by more or less neglecting their medical practice; and a few teachers who were paid for teaching other subjects and managed to slip in a little entomology on the side. But Riley was an out-and-out entomologist. He was widely known for publications that were well-written, beautifully illustrated, and based on accurate and extended observations. His series of nine *Annual Reports* on the insects of Missouri was destined to be epoch-making in the history of economic entomology.

Riley was a handsome man and a ready speaker. He was tall, slender, dark, and had the long flowing mustache of a buccaneer. He had a pleasing approach, a ready pen, and a still more gifted pencil. With his endowment as an artist went also an artistic temperament and a flair for showmanship. Comstock listened and learned; learned much about the way to present the results of his own studies to an audience, from the man who was in his day doing more perhaps than any other to teach the people that the study of insects is worthy of public support.

After this contact with Riley, Comstock was more than ever conscious of his own lack of training and experience. With the first summer vacation following came Comstock's first opportunity to sit at the feet of a great entomologist. Dr. Hermann A. Hagen had just been brought from Germany by Louis Agassiz to take charge of the insect collections in the Museum of Comparative Zoology at Cambridge, Mass. Comstock hastened to Cambridge to study with him.

No happier choice of a master in entomology could have been made. Hagen was then at the height of his powers. He was a great scholar, educated in the classics, well-acquainted with the whole range and history of entomology, and well-grounded in the field of morphology that was at that time coming into marked prominence. He was also entomology's first great bibliographer. However, he had more complete command of several other languages than he had of English. I remember that Comstock told me that when he and Hagen sat down at opposite sides of the little table over which came most of the instruction of that marvelous summer, Hagen began by saying:

"Do you shbeak Cherman?"

"No, sir."

"Do you shbeak French?"

"No, sir. Sorry."

"Do you shbeak Latin?"

"No, sir."

"Vell den," (with a sigh of resignation), "I guess ve vill half to shbeak English. Come now and I vill tell you some tings vat I know aboud entomolochy."

So the work proceeded, with Hagen lecturing and making rapid sketches on loose sheets of paper that lay before him on that little table, and Comstock listening and taking notes.

New realms in the field of entomology were being opened to him, far and away beyond those with which he had already made himself familiar. Between sittings he expanded his notes, and he told me many years afterward that he was still finding these old notes very useful in the preparation of his own lectures. It was with some pride that he said, "I was Hagen's first pupil in America."

As from Wilder he learned methods of comparative anatomy, so now from a distinguished specialist in his own field he learned morphology, and the impor-

tance of collecting and studying the life histories of insects along with their adult stages. From this master bibliographer he also learned much about how to deal with the infinity of detail that the study of insects imposes.

Then Comstock went for instruction to a man of great competence in economic entomology. He went to see Dr. Asa Fitch, the first state entomologist of New York, who was publishing excellent annual reports on the economic insects of the state. Dr. Fitch was a modest, kindly, scholarly gentleman, a country doctor, who, while practicing medicine, had built himself a little museum in his own dooryard. In this little retreat he kept his collections and did his writing. Comstock sat down with him there, and asked him about his methods of work. Fine as Fitch was as a student, investigator, and writer, he was not, like Hagen, born to be a teacher. When Comstock asked him how to study entomology, he said, "You just sit down with an insect and study it."

Comstock also went to Yale, where he found no entomologists, but he was glad to make the acquaintance of the zoologists Verrill and Hyatt. He also took advantage of excursion rates to the Centennial Exposition in Philadelphia. While there he spent most of his time at the Academy of Sciences, where he noted the fine library and large collection of adult insects. He also noted the (at that time, usual) lack of their immature stages.

The following year he received his first regular appointment to the teaching staff of the University as an instructor in entomology, and the course of his own life was set for the future. He began at once to build up a teaching collection of insects and thoroughly to acquaint himself with the entomological resources of the region round about the University.

In the second term of Comstock's senior year, Dr. Wilder was called away to give a course at Bowdoin College. He left Comstock to substitute for him and to give lectures in the regular course in invertebrate zoology to a class of 150 students. During this year one of Cornell's distinguished visiting professors, John Stanton Gould, taking note of Comstock's zeal and industry, presented the Department of Zoology with its first compound microscope, especially for Comstock's use. His best optical equipment up to that time had been a cheap low-power pocket lens. This microscope was substantial aid. It was wonderful progress, when, a little later, he was authorized to go to Rochester and purchase for his laboratory three compound microscopes.

Following Comstock's graduation at Cornell in June 1874, Charles V. Riley, who was then Entomologist of the U. S. Department of Agriculture, asked him to investigate the depredations of the cotton leafworm of the South. He went forthwith to Selma, Ala. This was his first real labor in the field of applied entomology.

That was the summer of a great yellow fever epidemic in the South. He spent it in the study of the life history and habits of this very destructive cotton insect. He saw how vast are the losses that an obscure little moth may cause, and how great the need of public support for the study of such pests.

In the winter of 1876 he made a trip to Florida to collect insects in a new faunal region. He went up the St. Johns River and spent a time at Ft. Reed where he collected diligently. He wrote home: "This is the richest entomological field I have ever worked." And he predicted that many species new to science would be found in the material he was gathering there. New species are still being described occasionally from that material.

In the spring of 1877 after his course

in entomology at Cornell was finished he gave a course of lectures on entomology at Vassar College on invitation of Professor James Orton.

In July 1878, he went again to Selma, Ala., to continue his field studies on the cotton leafworm. In the following September he stopped awhile in Nashville, Tenn., with C. V. Riley while they together prepared a quarterly report. Comstock then returned to his place at Cornell, and sent L. O. Howard, his most advanced and one of his ablest pupils, to be Riley's assistant in Washington.

An overturn occurred in national politics and Riley was dismissed from his position in Washington and Comstock was appointed Entomologist of the U. S. Department of Agriculture in his stead. Comstock got leave of absence from Cornell and went to Washington for two years. During this interval William Stebbins Barnard carried on the course in entomology at the University.

The woman who assisted in the establishment of the Department of Entomology in Cornell University, and who had a very large share in its success, was Anna Botsford. She was of Quaker ancestry and came from a good farm home in Cattaraugus County, N. Y. On the farm she acquired a lasting enthusiasm for outdoor life. Her father was something of a naturalist, and a good, intelligent, and worthy citizen. Her mother, Susan Allen Botsford, was a woman of refinement; and the two gave their daughter a gentle upbringing and a happy outlook on life.

Anna Botsford was socially gifted; she had the blessed habit of looking for good qualities in everyone she met. She was also a fine student, both of books and of human nature. When she arrived at Cornell in 1875 she was chiefly interested in English and in history; but in order to balance the subject matter of her studies she signed up for a course in invertebrate zoology. Thus young Comstock became her teacher. She found invertebrate zoology both interesting and informing, and she made a good record in the course. She next enrolled in Comstock's much smaller class in field and laboratory entomology. Here Comstock was at his best. Here she caught his enthusiasm for the study of insects. Here began their field trips together—many more of them, it so happened, than were on the class program. Happy days of further exploration followed, when together they climbed the hills and tramped through the gorges about the campus; went canoeing on Cayuga Lake[1] and on the flower-fringed bayous that meander through the Renwick woods at its head. Comstock had the haunts of insects in many interesting places to show her. So she began to share his interests.

The country about Ithaca in that day offered a fine opportunity for field work in natural history. The campus was as yet little more than hilly woodland farm, with the teeming insect life of shady ravines all about its doors. Its nearby lakes and streams, its hills and valleys, its waterfalls and deep gorges, its upland bogs and lowland swamps, each had its share of the rich fauna and flora of the Finger Lakes region. The shy and dainty trailing arbutus could still be found growing in places that later became parts of the main quadrangle.

The Cornell campus, lying as it does, outspread on a terrace of the broad East Hill of Ithaca, is bounded (as is well-known to all) on two sides by swift streams that break into alternating waterfalls, plunge basins, and riffles all the way down the hill; Fall Creek on the north and Cascadilla Creek on the south

[1] A gem from Anna Botsford's diary: "The day was perfect; a soft Spring haze covered the hills, and the lake was so mirror-like that two wild ducks swimming across it made a W that spanned half its width."

side. On the high ground at the northeast corner of the old quadrangle beside the Fall Creek gorge Comstock "staked a claim." He obtained from the University Trustees a long-term lease on a piece of ground there, and built a house on it. It fronted on East Avenue, and from the front there was a superb view of lake and valley and the distant, checkered fields of West Hill. There were tall oak trees growing on the steep slope down to the gorge; trees that reached their long arms so close to the windows of the house that, looking out from them, one seemed to be in the treetops among the birds and the squirrels. And there was the low sound of running water coming up through the trees from a waterfall far down below. A cultivated garden was at the rear, and, an orchard; and farms beyond the orchard. What a wonderful environment for two naturalists! They were married in October 1878.

The remainder of this narrative is a story of teamwork in education.

The Comstocks began housekeeping in their new home on the campus. Mrs. Comstock records that the first thing she had to do was to learn how to cook, and that her first pies were not a success; but that they were very happy. Soon they were working together across the quadrangle in McGraw Hall on a report of his last season's field work on the cotton leafworm.

Out of a clear sky came a call to Washington. As a result of a political overturn, Riley was out, and Comstock was invited to come to Washington and take his place in the U. S. Department of Agriculture. They were loath to leave their new home; but to a young assistant professor, married, and living on a yearly salary of $1,000, working in a new and little appreciated field of university interest, the call to the top entomological position in a great Department of the national government seemed a golden opportunity. In Washington he would have a budget of $5,000, a salary for himself of $2,000, with two paid assistants: L. O. Howard at $1,200, and Theodore Pergande at $750; and he could employ George Marx as an artist at $5.00 per day. So they went to Washington. They took with them, very properly, the unfinished manuscript on the cotton leafworm, and there expanded it into the well-known *Report on Cotton Insects*.

They found the office of the entomologist lacked a compound microscope. There was a Division of Microscopy then in the Department of Agriculture, and anything too small to be examined with the unaided eye had to be taken over there for study. Comstock bought a microscope. He also spent $200 of government money for a Remington typewriter that is said to have been the first used in the Department of Agriculture.

Before the report of cotton insects was out of the way there came insistent demands from Florida and California for help in control of citrus insects, and Comstock and Howard began a general investigation of them. Soon Comstock became aware that he needed to see the general situation in the field, and he went to Florida for field work in the orange groves. Mrs. Comstock stayed in the office to keep things going. She attended to the correspondence of the office, writing letters first in longhand, and then on the typewriter, after she had learned to use it. She also typed and filed her husband's field notes, and made drawings for his reports. She distributed silkworm eggs to 120 applicants who wanted to assist in an experiment at silk culture in America, and she did whatever else the situation demanded.

Later Comstock made a trip to southern California, with the same objectives and for a longer stay. There he did the field work on which was to be based a

report on scale insects that was destined to be one of the outstanding documents of economic entomology. It was reprinted in 1916 as a Bulletin of the Cornell University Experiment Station.

It is not for me to try to tell the story of the struggles of the office in Washington to keep up with the nation's growing demand for help on insect problems. That story has been committed to writing in a biographical manuscript by Mrs. Comstock, which is still unpublished.

After two years in Washington another political shuffle brought a new head to the U. S. Department of Agriculture. Dr. C. V. Riley was restored to favor and reappointed to his former position. Comstock was out, but an equitable arrangement with him provided for continuing his pay until completion of the reports he had in hand, and for their proper publication thereafter. Dr. Howard stayed on in the office as Riley's assistant; stayed for a distinguished career as Chief after Riley's death.

In the two years at Washington Comstock had made contact with the principal insect problems of the nation at large. He had widened his own horizon by travel. He had acquired some useful, if disappointing, knowledge of how the scientific positions in the government service were peddled about in his day by the politicians. If he felt himself limited by a return to a position in which he could command lesser resources, he expressed no disappointment. Quite the contrary. Just before leaving Washington he wrote to his wife, who had returned to Ithaca earlier:

> We will take up the work at Ithaca with confidence. We will have a happy home. We will give my students the best facilities for obtaining an entomological training that can be found in the world. And we will do some original scientific work. . . . Just now I am at work on a plan by which the students, after the first term, will do original scientific work. In that way I shall have a large corps of assistants and they will get the best of training.

At Ithaca, White Hall had been completed during his absence, and he was given greatly enlarged quarters on the second floor. He now had an office, a lecture room, and a separate laboratory. He had a telephone installed in his office, one of the first on the campus, and a typewriter. He had cabinets built after his own design, to hold insect cases, with enough space in them for large future collections. He inaugurated the system, now in general use, of keeping his pinned insect specimens in glass-topped cases pinned to movable blocks. This provided for easier rearrangement, and the interpolation of new material without the necessity of repinning the specimens.

He returned eagerly to teaching. Besides the large course in invertebrate zoology that he inherited from Doctor Wilder, he had a class of twenty students in entomology. And in the spring he took on a new course in apiculture in which seven students enrolled.

From the beginning he gave his students in entomology all the field work that the strict hour-limitations of the academic program would allow. His pedagogical procedures do not appear to have been standardized as yet, for he made a class exercise out of the cutting of a bee tree in the woods at night, getting out the honey, and luring the bees into a box hive. And (doubtless at his own personal invitation) President White and Professor Goldwin Smith went along as spectators!

He spent part of a summer vacation in bee-tree hunting and in trout fishing. His hunting was not so much for honey as for more knowledge of the ways of the bees in the air, and their homing instincts. With a keen eye he followed single bees from their feeding places, as they wheeled away and swiftly disappeared. Then he followed lines of bees onward in a straight course to their home in a hollow tree.

After finishing his report on the Coc-

cidae (scale insects), along with which went Dr. Howard's report on their parasites, he began the preparation of a textbook for the use of his classes. He built it from the ground up, basing it on materials that his students could see and handle. He gave his keys for insect determination the severe test of repeated use by beginning students, and he revised them again and again, always striving for simplicity and clearness of statement.

His aim was, and continued to be until the end of his teaching, to have a basic course in general entomology, from which later specialization might proceed. After mastering the fundamentals in this course his students could specialize to their heart's content. He began to accumulate an entomological library with the same regard for essentials.

Then he got an insectary, the first of its kind. I recall that he once told me with great glee how he got it. For several years he had been hinting to the administration that he needed a building in which he could keep living insects under observation. Professor I. P. Roberts, then head of the Department of Agriculture at Cornell (not yet a college), called him into the office one day and said: "There is an unexpended balance of $2,500 in my fund. It will lapse if not used before the end of the fiscal year. That is six weeks from now. If you can get your bug house up and get the bills paid in six weeks you can use that money."

Comstock leapt to the occasion. He drew the plans for the building that night, but not, of course, without much previous forethought; staked out the ground for it next morning, and got the work of excavation going next day. The building started a few days later. It was completed almost on time, and by private arrangement with reliable workmen, he had the bills all in and paid by the end of the fiscal year—a plain case in which the law was "more honored in the breach than in the observance."

I am amazed when I think what he got for the money: a two-storied head house with a two-section standard greenhouse in the rear; in the head house, four large rooms, two on each side of a central stairway, and steam furnace and toilet in the basement; in the greenhouse, hot and cold sections, steam, water, and gas!

One room on the second floor was assigned to a student who, in return for its use, took care of the building. A long succession of budding entomologists came to occupy that room, one of whom (Stocking) years later became acting dean of the College of Agriculture.

The insectary was located well back of Comstock's house between the woods and the garden, where its lack of ornamentation did not matter. It was built for service, not for appearance. It sheltered some of the most important work done in entomology during two generations.

Comstock was now equipped to take advantage of the rising tide of interest in entomology, so he pushed ahead. But his alloted space in White Hall was becoming inadequate. It was crowded with students and accumulated teaching aids. The equipment for photography that he had to have near at hand for use in his own research and in that of advanced students he squeezed in by applying his wits to economizing space. For darkroom use he adapted a small closet, equipped it with lights, running water, and shelves high and low, on which everything needed was in reach from the middle of the floor. He bought a bulky camera with rectilinear lens.

The operating table was a contrivance of his own that every student of its day will remember. It was wide enough only to carry the big camera and long enough for full extension of its very long bellows. It was high enough for Comstock's own convenience when standing erect

while focusing; taller persons had to stoop. On it sat the camera perpetually, under a shroudlike sheet of black cotton-flannel, that served as a focusing cloth while in use and as a dust cover between times. Underneath was a built-in storage compartment in which accessories were kept: plateholders and kits for glass plates of various sizes (it never knew films). It stood in a corner by the closet against the wall. It was rolled out on its own casters for use when no class was occupying the room. It saw great service.

Meanwhile Comstock's publications were winning for him high professional standing. His *Report on Cotton Insects* brought letters of congratulation from entomological colleagues and others. Especially appreciated by him were two of the letters commending it, one from his master in entomology, Dr. Hagen, and one from Charles Darwin. When his report on the Coccidae appeared it became at once the standard reference work for students of the scale insects the world around.

He was invited to join the editorial staff of the *American Naturalist*, and for three years he edited a section on entomology in that journal. He also wrote articles on insects for the *Standard Natural History*. He answered queries about insects for several agricultural journals; and he appeared more or less regularly for an address before the annual meetings of the New York Horticultural Society.

(*To be concluded*)

AN URGENT MATTER

After almost one hundred years of existence as the over-all scientific association of this country, the A.A.A.S. finds itself choked by its own growth (see pages 127–130). The officers of the Association do not have time to give the attention to the campaign for a building fund that it deserves. We hoped that our members and friends would recognize our plight without repeated reminders and take action on the merits of the case as presented in our prospectus. Many have done so, but many more have not yet contributed. May we suggest to those who have contributed that they do what they can to persuade others to contribute and that they find out whether their own affiliated societies could not contribute from their permanent funds as the American Association of Economic Entomologists did at its December meeting in Dallas, Texas? Members who are employed by commercial laboratories should also see the officers of their companies and point out that their Association is in danger of bogging down.

To those who have contributed to the centennial building fund we are extremely grateful, but we hope that they will not be satisfied until they have used every means of persuasion on every prospective contributor whom they can approach.—F.L.C.

CASTE, CLASS, AND COLOR IN INDIA

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CASTE, as a social institution, is not peculiar to India. Throughout the ages all societies have divided their people into various groups on grounds of particular traits and talents, equipment and opportunity, character and culture. And when such groups pursued their particular ideals and professions to the entire neglect of other groups' interests, they tended to grow up as isolated, individualistic, and even mutually antagonistic units in the same society. Nevertheless, only in India did such broad divisions of mankind crystallize into a code and an ethic which govern and dominate every detail of the workaday life of particular groups.

Once these divisions, based on individual and group tastes, traits, and talents, were effected, occupational and professional differences strengthened them even more, for the fact that one kind of occupation was more lucrative than another led to economic differences which became yet another prop of the caste system. And, human nature being what it is, it became common for the son to follow his father's profession. Consequently occupational differences became hereditary. Thus birth and occupation became the twin basic principles on which the caste system in Hindu society rested and revolved.

In the course of some centuries an elaborate code of conduct affecting meals, migration, marriage, and morals came to be drawn up. Later centuries added further ramifications pertaining to ceremonial purity and pollution, commensal restrictions, connubial taboos, unapproachability, and even untouchability. To the original basic principles of birth and occupation were added heredity and endogamy, and the subsequent development of caste has rested on all these four attributes. The result is that today India's caste system has become a complex and irrational system, defying all classification.

This does not mean, however, that the caste system is a rigid, static institution persisting in a society where the need for it has vanished. On the contrary, it has been dynamic, but only to the detriment of India's national growth. Whereas the caste system, at its inception, recognized only four or possibly five divisions, based on the essential fourfold or fivefold functions in a primitive rural economy, today the system has divided and subdivided itself to such a profuse and alarming extent that about 3,000 castes have been enumerated in modern India; and new castes are in the process of formation even today! Thus the popular supposition that caste is immutable is erroneous. On the contrary, it is and has always been susceptible to change at the slightest pretext of altered social conditions. This change has been highly unhealthy, for it has led to endless segmentation of Indian society.

Several factors have contributed to this interminable fission, such as change in occupation (usually from the lower to the higher), adoption of a new or the abandonment of an old religious principle or social custom, or merely increased prosperity leading to further gradations of economic equality. Despite these qualifications, caste in theory must be explained largely in terms of: (a) an unchangeable "social inequality" based on birth; (b) the gradations of the professions and their inequality; and (c) the restrictions on marriage outside one's own group or caste. Change in occupation, however, is the primary fac-

tor that creates new castes in rural India, even today.

Keeping all these attributes in view, we may define caste in India as an endogamous group or collection of endogamous groups, generally bearing a common name, membership of which is hereditary, imposing on its members certain restrictions in the matter of social intercourse; either following a common traditional occupation or claiming a common origin; and generally regarded as forming a homogenous community.

The Origin of Caste. Several theories have been put forward to explain the origin of caste. The one hypothesis on which there seems to be most agreement among sociologists explains the genesis of caste as an outcome of the clash of cultures: In the dim past, at a time which may roughly be called the pre-Vedic period, a tall, fair-skinned people inhabited central Asia. While trekking in search of food and a new habitat, they entered India through the northwest frontier passes. These incoming "Aryans" had no caste system as such, but they recognized three broad functional divisions in their own society: the priests, who offered prayers and performed sacrifices; the chieftains and soldiers, who fought in battle; and the rest, who, although normally nomadic, sometimes plowed the fields and raised food. Soon these newcomers began to move into the interior of India, eastward toward the Gangetic Valley, where several Hindu kingdoms were then flourishing. Increasing in numbers and constantly moving in search of shelter and sustenance, these new arrivals inevitably clashed with the original inhabitants. And in the eyes of these newcomers the indigenous peoples seemed uncivilized. Wars were waged and the newcomers gained what should roughly be called "political control" in parts of India. Then, when these two groups of people, culturally alien, settled down to live side by side, marriages, both approved and unapproved took place. Each culture-group tried in vain to maintain its so-called racial purity. The ultimate result of this anxiety to safeguard the conflicting group cultures was the caste system.

On account of certain stray references in the writings of the period to the "fair-skinned" incoming Aryans and the "dark-skinned" native inhabitants of India, some scholars have jumped to the conclusion that caste was originally based on *Varna Bheda,* or the difference in color. But neither cultural antagonism nor color alone can explain the origin of caste, because four castes based on vocational differences soon came into existence.

Another hypothesis was advanced by Sir Edward Blunt, who implies that the caste system was imported into India by the invading Aryans. Moreover, he also emphasizes the color bar, which, he believes, created an endogamous tendency that developed into a rigid custom. It is his theory that the Aryans were already socially organized into three classes: the *Kshatriya* nobility; the *Brahmin* priesthood; and the *Vaisya* commonalty. The intermarriage that did occur with the indigenous race produced a fourth class, *Sudra.*

This hypothesis is untenable, for it does not explain how the Aryans themselves came to have three castes before they came to India, and why, if the first three castes were Aryans, these groups do not intermarry or even interdine. Nor does this analysis explain the fact that there is no mention of caste in Vedic hymns. According to Professor Max Müller:

> There is no authority whatever in the hymns of the Veda for the complicated system of castes. There is no law to prohibit the different classes of the people from living together, from eating and drinking together; no law to prohibit the marriage of people belonging to different castes; no law to brand the offspring

of such marriages with an indelible stigma. There is no law to sanction the blasphemous pretensions of a priesthood to divine honors or the degradation of any human being to a state below the animal.

Most Indian scholars agree with the theory of the French scholar Senart. After a comparative study of Aryan institutions in ancient Greece, Rome, and India, Senart concludes that caste was the Aryan answer to the problem of culture contact. He believes that when the Aryans penetrated into northern India they met with a "race" alien to their own culture and character; and the consequent anxiety of the Aryans to preserve their "racial purity" in addition to the distinction between conquerors and conquered led to the demarcation of their own people from all other people.

Were this the true explanation, there would be only two castes, the rulers and the ruled. Although the fact that "higher castes" form only a small minority of India's population might seem to support this view, how can we explain the absence of castes among later invaders from the Moslems down to the British? Furthermore, this view also ignores how a small minority—the *Brahmins*—who were not rulers, achieved social superiority in northern India. According to Senart, the conqueror or ruler should constitute the highest caste, but in the existing caste system it is the *Brahmin* priest who is above the ruler. Moreover, Senart's theory does not explain the all-important fivefold divisions of society based on occupational differences.

All these surmises about the origin of the caste system being found in an inherent Aryan-Dravidian conflict are not only highly unreal but are sharply in contrast with our modern anthropological evidence. As Professor S. Radhakrishnan points out:

The system of Caste is in reality neither Aryan nor Dravidian but was introduced to meet the needs of the times when the different racial types had to live together in amity. The only way of conserving the culture of a race which ran the great risk of being absorbed by the superstitions of the large numbers of native inhabitants was to pin down rigidly by iron bonds the existing differences of culture and race. Unfortunately this device to prevent social organization from decay and death ultimately prevented it from growing.

Although Professor Radhakrishnan appears to come closer to the truth as we see it, it is not clear what he means by "race." Are we to presume that some five "races" came into India and were rechristened as five castes; or was caste merely the ancient Hindus' passion for labeling professional and ethnic groups of people?

Although there is no denial of the grains of truth contained in the various hypotheses, neither cultural antagonism nor "color" alone can explain the origin of caste, and we must seek elsewhere for a more logical explanation for the origin of the Hindu caste system—one that will explain the divisions into four groups based on professional or vocational dissimilarities. Perhaps, with the development of group consciousness in India and the consequent desire to end the social anarchy and confusion of a small primitive society, some self-chosen group leader deliberately planned a division of labor. The need for worship and the love of colorful ritual would have created the necessity for a class of clever men who would officiate as priests and propitiate the gods. The need for an army or police would have created the second class. The need for exchange and barter would create the third; the need for raising food, the fourth. Originally there would have been no question of superiority or inferiority. Even the *Panchamas*, the fifth class, composed of those who had no tastes and talents for anything but menial labor and so were assigned to do the cleaning and scrubbing, sweeping and washing, would not have been looked down upon. Thus *at the*

beginning everyone chose his vocation on the basis of ability, inclination, and aptitude.

In the course of centuries, these occupational groups lost their elasticity and mobility and tended to become inflexible. The son of the *Brahmin* priest became a priest, and the son of a *Panchama* sweeper became a sweeper; the hallmark of caste became birth and heredity. And now the priest, who took two baths every day in the nearby river to be ceremonially clean before he stepped into the sanctum sanctorum of the Hindu temple, did not think much of the *Panchama* sweeper who was externally unclean. Human nature being what it is, marriage among members of the same professional group became common. And we can understand why sometimes the daughter of the *Brahmin* priest refused to marry the son of the *Panchama* sweeper, and vice versa. It was thus that endogamy became an additional principle of caste. This, to us, is roughly the genesis and growth of caste.

The conversion of this originally elastic system, after a long stretch of time, into a rigid order based on birth and heredity, with commensal and connubial restrictions, does not seem so absurd even in the light of modern conditions. This is not a defense, for there can be no rational defense of caste, but a description of these restrictions. In other countries there are parallel restrictions, less rigidly defined perhaps, but existent nonetheless. Let us look at the West through the eyes of George Bernard Shaw:

Suppose a middle class British mother said to her daughter, Miss Smith or Miss Jones, "Follow the promptings of your heart my dear, and marry the dustman or marry the duke, whichever you prefer." But she cannot marry the dustman; and the duke cannot marry her, because they and their relatives have not the same manners and habits; and people with different manners and habits cannot live together. And it is difference of income that makes a difference of manners and habits. Miss Smith and Miss Jones have finally to make up their minds to like what they can get, because they can very seldom get what they like; and it is safe to say that in the great majority of marriages at present, nature has very little part compared to circumstances.

Exceptions to this general tendency of social immobility in marriages are not wanting in modern democratic countries, but they are not frequent. We do not find every day a king marrying a commoner or a duchess marrying her chauffeur.

As for Hindu commensal restrictions, Western parallels are not wanting either. It is true that a *Brahmin* priest does not break bread with a *Panchama* sweeper, nor does a *Sudra* peasant pour a cup of tea for a *Vaisya* trader. This is nothing more than the Western social custom—or snobbery, if you please—that forbids the parlormaid to sit down in your drawing room or prevents your asking the gardener to dinner. True, these modern analogies do not go on all fours. Citing these Western parallels to explain caste may seem as if we were confusing the Western class system with the Hindu caste system. We are considering caste in a fossilized state, centuries after its meaningful inception. But if the system originally was nothing more than a class system or a division of the people according to their primary occupations, how can we account for the hundreds of castes which exist today? The answer of the Indian scholar S. S. Nehru is worth quoting:

To understand the rationale and raison d'être of caste and its place and function in rural economy, let us make the following hypothesis: If there had been no industrial revolution in England; if the struggle for existence in that island, which is geographically a unit but economically the reverse, had been less keen than it is; if the passive acceptance of life on any terms had smothered the inventive genius of the islander; if the brotherhood in arts and crafts had continued to guard their secrets jealously; if their principle had been accepted and expanded to include all rural professions and social services; if the guilds and similar associa-

tions had been armed with dire sanctions in the way of forbidding vis-à-vis others such as intercourse, interdining, intermarriage, wherewith to keep their membership economically, professionally, socially entire; if the watertight compartments created and fostered in that way had continued to accumulate traditions like a dead weight to retard their otherwise healthy growth; if the cleavages of such bodies became more and more acute and the linkages correspondingly less and less firm, in course of time—

Why, then England too would be having a caste system quite as strict and complicated as any in India today. The artisans and craftsmen, the butcher and botcher, barker and baker; the carrier and cartwright and cowherd and cutter; the dyer and dustman and dairyman; the farrier and fowler and furrier and fuller; the grocer and goatherd and gardener; the harper and hawker and hatter; the joiner and jeweller and Jewish money-lender; the kitchener and knocker; the knacker and launderer; the minstrel and mason; the oilman and oxherd; the pursuer and poacher and pastry cook; the quarryman and collier; the squire and scrivener, salter and saddler, sweep and stonecutter; the tailor and tinker; the vintner and vagrant; the woodman and watchmaker; the yoeman and yokel—briefly told, all these categories from A to Z of human activity in the social economy would be developed into castes, each living and working in his own watertight compartment, hermetically sealed and cut off from the rest, and then the English castes would be quite as numerous as the Indian castes in any given rural area.

Such in brief is the genesis and growth of caste as a social institution in India—the child of the Indian or Hindu primitive efforts to run "society" on an orderly and systematized basis—if we can stretch our sociological imagination to the dim beginnings when "society" itself was being born in India.

Evolution of Caste and Class in Modern India. Caste in modern India is an example of social institutions that have developed almost to be the opposite of what they were intended to be. There is a tremendous gulf between what caste was planned to be and what it actually has grown to be. There is no doubt that caste in Hindu society was created to meet a definite need, although sociologists may disagree on what the precise need was. As originally conceived and practiced, it had no class consciousness and economic implications. It never pursued values in terms of rupees and annas. It recognized no snobbery of worldly power or prestige, for the first caste was composed of priests who were never wealthy. Nor was the vocational division strict and rigid originally, for *Brahmins* have been warriors and doctors from earliest times. The Maurya emperors were *Sudras,* and several princes in India today trace their genealogical line to *Sudra* ancestors. In fact, we read in the *Puranas* stories of individuals and of families who changed from lower to higher castes and vice versa. Manu, the Hindu lawgiver, admits the possibility of ascent and descent. The *Mahabharata,* one of the great Hindu epics, points out:

> There has been so much mixture in marriages that no test of *jati* (or caste) or birth is good. The governing consideration should be *sila* (or character) and the first Manu has declared that there is no point in distinctions of caste if character is not considered.

According to the great Hindu scripture *Bhagavad Gita* (The Song of the Lord), "The fourfold division of caste was created by me (the Creator) according to the apportionment of qualities and duties. . . . Not birth, not sacrament, not learning makes one high caste but righteous conduct alone causes it."

The creation of caste, as we have seen, had nothing to do with the Hindu religion. Once the system came to stay, however, the Hindu religionists seized it, rationalized it, and gave it a metaphysical and theological color, which to begin with did not distort the real purpose of caste. Despite all the religious injunctions that behavior and not birth and belief should be the hallmark of caste, the lapse of centuries blinded Hindu society to the true nature and significance of caste. It became stratified and

rigid and fell a victim to the abuses against which it had been warned. Moreover, a philosophy was created and a rationale was evolved by the group consciousness of the first caste to support the myth that caste was divinely ordained. They spread the insidious dogma that the status of every individual in this life and the caste in which one was born were determined by *Karma*, or one's actions in previous birth. With interpolations such as these, the abused caste system was foisted upon an otherwise innocent Hinduism. Hinduism cannot be blamed for this social fragmentation any more than Christianity can be blamed for the present international anarchy. The masses were misled, and the gullible millions resigned themselves to a system that fettered them to myriad disabilities. And as wave after wave of foreign invasions reduced India to a life of constant peril and insecurity, these fissures in Hindu society became deeper and resulted in the fragmented shambles of the meaningless social structure that caste is today.

Caste and Color. Some European and American scholars have tried to explain the Hindu caste system as the outcome of the contact and conflict between the alien *light-skinned* Indo-Aryans and the Dravidians, the original and older inhabitants of India. This might have been expected from Nazi Indologists who can explain institutions they do not comprehend only in the light of race problems based on color. Is there any basis for this comfortable myth of certain Western scholars whose preoccupation in this regard seems to be the pigment of the skin? Since the *Vedas* mention *Arya Varna* and *Dasa Varna*, and the four supposed high castes as *Chatur Varna*, and since *Varna* means, among other things, color, these observers have readily jumped to the conclusion that the caste Hindu was white and the outcaste Hindu was black. Were we to accept this, the logical absurdity must follow that the second caste must be red and the third yellow and so on. These scholars would like to think naturally of white *Brahmins*, red *Kshatriyas*, yellow *Vaisyas*, brown *Sudras*, and black *Panchamas*. But there was never a "race" problem in India in the scientific anthropological sense. The castes in India, therefore, do not represent 4,000 or 5,000 different biotypes with as many shades of color beginning with black and ending with almost blond. *Caste is not physical and is not and was never based on color.*

The problem of the "untouchables" has often erroneously been treated as a separate problem by several writers. Caste and untouchability are not two different social institutions but are merely the two sides of the same counterfeit coin. They are indivisible, for when we have caste we naturally come to have the outcaste too. The outcaste is the logical corollary of the caste and as such must be approached as an integral part of the caste system. The only and the best way to abolish untouchability is to abolish caste. We cannot destroy one phase of a sickly social institution and retain the whole. Gandhi once said that if Hinduism is to live, untouchability must die. And if the caste system lives untouchability cannot die. A quick and effective method for its abolition should and must be found.

Caste today. Caste has become something new. It is neither what it was intended to be nor even what it was a few decades ago. In theory the system and its restrictions do exist, but their sting has gone. Countless new, nation-building forces, both from within and from without, have set in. The resulting disintegration has reduced caste in several regions of India to a ghost of its

former self. Most vital of these forces have been Western education, modern democratic conceptions of equality, rapid means of transportation, increasing urbanization, and gradual industrialization of the country. The influence of Western education has led intelligent people to wonder about the need for caste today. Happily, many of these progressive forces attacking caste have come from within. All social reformers who have attacked caste, from Raja Rammohun Roy to Mahatma Gandhi, have belonged to the Hindu fold.

Roads, buses, railways and steamships, airplanes, telephones, the telegraph, and radios have begun to reduce India's geographical immensity to a tiny unit. Interprovincial migration, rural exodus, the growth of cities, emigration and foreign travel—all these are breaking down the barriers of caste. Industrialization is upsetting the traditional pattern of the occupational distribution of India's population.

To take up only one caste as an example, the Brahmins, the supposed *Herrenvolk* of Hindu society, can be found today in almost all occupations. Their ancient monopoly of priestcraft and piety has vanished. They follow liberal professions, such as law, medicine, teaching, and Government service — from clerks to ministers of the cabinet. Some Brahmins are landowners, or *mirasdars*. Some have become peasants. There are Brahmin nonpolitical convicts, and there is actually an all-Brahmin criminal tribe, like the *Tagris*, on the upper Jumna.

The same is true, for instance, with *Sudras*, the peasants who belong to the supposedly high-caste Hindu society. Today they are found not only in their traditional occupation of farming but in practically every other profession as well. Among them are doctors, lawyers, professors, presidents, clerks, civil servants, businessmen, bankers, cabinet ministers, Sanskrit scholars, and even priests.

The influence of caste on occupational distribution has definitely vanished. The inevitable pressure of modern economic insecurity has forced the people to forget *Manu's* injunctions and earn their living as best they can. No longer can it be strictly said that caste interferes with the economic life of the people. The driving force behind present-day occupational distribution is ability, opportunity, and qualifications, not socioreligious stratified gradations of a bygone era. Today we find priests and professors, doctors and lawyers, cooks and maids, beggars and vagrants among all castes. The caste system has become metamorphosed into a class system—the haves and the have-nots.

Yet in parts of rural India, in meals and marriages, caste restrictions do continue even today. Intercaste dining and intercaste marriages are increasing, but they have not become the order of the day. Nevertheless, the people are realizing that their caste prejudices are undemocratic and hostile to a healthy nationhood. Many a blow at the caste system has been struck, but as yet none has proved fatal. But caste must and will go. And the hastening of its end is the endeavor of young and progressive India.

OUR INDIGENOUS SHANGRI-LA

By WARD SHEPARD

IN THIS century of revolution, the world will be swept by social changes as vast and far-reaching as the technological revolution of the past century. Fundamentally, the emerging social revolution—of which two world wars and a world depression were symptoms—revolves about the nature of man and of human society. Peoples are stirring to achieve a freedom, a dignity, and a way of life worthy of their status as human beings. Can these changes be scientifically guided as were the vast technological changes of the past century, or must they be left to the emotional clash of partisan politics and of pseudoscientific ideologies? Can we achieve a science of society, can we learn to diagnose the spiritual maladies of modern man, can we build life-giving institutions which will release the greatness that is potential in all men, can we define valid goals of collective social endeavor that correspond to the actual spiritual nature and needs of man? Or must social reconstruction continue to be based on rules of thumb, hunches, guesses, and pseudosciences?

The answer hinges on whether or not it is possible to create a social science which, in the unique realm of human life, achieves the essential practical function of all science—namely, prediction. It is the predictive power of science that gives men some prevision and some control over their destiny. Measured by this test, the social sciences have been highly deficient. Eddington has said that a physicist cannot but look with dismay on the disorganization of the sciences pertaining to human life. This disorganization grows out of the failure of the social sciences to achieve qualitative methods of assessing the values of human institutions. Historically, under the impact of the "natural law" doctrines of the Enlightenment, the great quest of the social sciences was for a system of "natural laws," modeled on the classic Newtonian concept, externally governing the origin and development of human societies. In its heyday, Comte and Spencer were the philosophical interpreters of this quest and, in its decline, Spengler. With the recession of the rainbow of "natural law," the social sciences turned to the useful, but not very illuminating, task of description, and at the same time cultivated a pose of moral neutrality toward the social phenomena they investigated. If, however, the social sciences are to yield the qualities of prediction, evaluation, and control, they must expand their horizons and create new scientific methods.

The social sciences are confronted with a unique subject matter. In essence, it is the problem of human values. Human societies are what men make of them. Theoretically and historically, their diversity, good and bad, is limitless. What is a good society and a bad society, a good institution and a bad institution? If the social sciences cannot evaluate social institutions, then in this age of unprecedented whirling change the fateful decisions of world social reconstruction must be left at best to benevolent amateurs and at worst to dangerous ideological demagogues.

In this day of Armageddon, the last place in the world one might expect to find the clue to a science of society would be in a study of the ancient Hopi Indian villages, perched high on their sun-drenched crags in northern Arizona. Yet a fascinating little book, *The Hopi Way,* published by the University of

Chicago Press, and written by Dr. Laura Thompson, anthropologist, and Dr. Alice Joseph, psychiatrist, boldly and successfully attacks this central human theme: What kind of values does a given human society foster, how does it foster them, and what human personalities emerge?

In this article I have set forth the main conclusions of the authors of *The Hopi Way* and have accepted them at face value. But the broader interpretation of the significance of the new methods, as well as the conclusions on the philosophical implications of the Hopi way of life for modern society, are my own.

Historically, the endless struggle for human freedom has been the instinctive quest of multitudes of men for personal fulfillment. The kind of people a society produces is the only fixed bench mark against which the validity of a society and its institutions and values can be judged. Neither historical perspective nor the classical "natural law" myth affords any such fixed point of reference. The judgment of social institutions, and more particularly the projection of valid new institutions, against this bench mark will be the future field of the social sciences. The social sciences cannot continue to take refuge in the neutral pose, the amoral attitude, of physics. For all human purposes, the cosmos is fatefully teleological and the ultimate task of a science of society is to evaluate human values.

The Hopi Way pioneers boldly on this new path. In essence, it disentangles the reciprocal flow of forces to and fro between the totality of a society and the personalities of its constituent members, in order to weigh and value the kind of people that society produces. Scientifically, it is of secondary importance that it happens to deal with the Hopi Indian community of northern Arizona, as part of a larger study of several changing Indian tribes.

I have had the privilege of reading in manuscript the second volume of this series, on the Navajo people, by Dr. Clyde Kluckhohn and Dr. Dorothea Leighton. This extraordinarily vivid, detailed portrait of Navajo society and Navajo personality structure—both at almost opposite poles from the Hopi—strongly verifies my main conclusion from the Hopi study: that in the Indian personality project a new and powerful tool of social and personal analysis and evaluation is being forged. There is no intrinsic reason why the striking methodology applied to this small primitive group cannot be developed for great modern societies. Indeed, I should like to suggest that it be applied to a thoroughgoing analysis of the historic and social origins of the fatal psychic breakdown of the German people, in order to afford a scientific foundation for the Allied occupation and administration and for the long-range moral regeneration of the German people.

There is, too, in this little book the fascination of discovery beyond scientific pioneering. For the Hopi community, subjected to an intense and manifold scientific scrutiny, turns out surprisingly to be an "ideal republic," a pure, achieved democracy, intensely nurturing an ancient spiritual culture, intensely nurturing and socializing its young. And furthermore the unusual wisdom and the beauty of the Hopi way of life contain a healing message to minds drenched in the terror and pity of world tragedy, oppressed by the specter of vast and unpredictable change. Even more, the Hopis, having long since mastered the fine art of cultivating the garden of human life, have much to tell us about the essential eternal values required for the sustenance of the human spirit.

In their forbidding and picturesque desert, the Hopis have lived for centuries, and continue to live, largely iso-

lated from the great world. Living in several compact villages, with a total population of about 3,500 souls, they afford ideal conditions for a study of almost laboratory precision. Their culture is self-contained, complete, and remarkably stable. It is colorful, rich, and spiritually powerful. It is highly complex, but its complexity is offset by the fact that its categories of order are precisely organized and articulated into a singularly balanced whole. Finally, it displays virtually none of the pathological symptoms that have disintegrated modern civilization. It is a society devoted to intensity of living and to a sophisticated faith in man's importance in the cosmos.

The Hopi culture has developed and survived for over a thousand years against a background of drastically severe and precarious economic conditions. Food and water must be wrested with infinite skill from a semiarid land, subject to severe droughts, crop failures, floods, and, in recent times, soil erosion. Famine, through the centuries, has been an ever-present threat, and certainly often a grim reality. Yet the Hopis met and mastered the challenge of the desert both on the physical and on the social and the spiritual level. On the physical side, the desert forced them to achieve a remarkably effective technology of dry farming. On the social level, it forced a democratic, cooperative social structure which tolerated no waste of human energy and no individual self-seeking, and yet achieved a high degree of human freedom and individual development. And on the spiritual side, this starkly demanding earth-environment, which permits no negligence and no mistakes, yet yields reluctantly to precision, insight, and cooperation, may well be the ultimate source of the central philosophical theme of the Hopi religion and world-view. The entire realm of nature, say the Hopis, in its farthest cosmic sweep and through human society which is a part of nature, constitutes a system of reciprocal and mutually supporting relations, in whose working out the decisions, actions, and even the thoughts of men play an important part. Thus, the "primitive" Hopis anticipated by several centuries some of the main philosophical doctrines of James and Whitehead. Verily, among the Hopis, as among the ancient Hebrews, the stark clarity of the desert is more provocative of cosmic insight than the steaming jungle of industrial civilization.

The most striking thing about the Hopi culture is the almost complete absence of political government—a void that is filled by an extraordinarily complex, integrated, and autonomous system of social and personality controls. There is, to be sure, a modern Tribal Council fostered by the Indian Service, but it has as yet assumed little or no tribal leadership. And there are village chiefs and priests, whose main functions, however, are concerned with the traditional religious ceremonies. There is no lawmaking body: the law is custom, and custom is the law. Looking at the world trend toward centralized bureaucratic government, one would expect in the almost literal "anarchy" of Hopiland a perpetual reign of chaos. On the contrary, quite the opposite is true. The principle of order is implicit in the organic structure of the total Hopi culture. This culture is based on the insight that freedom and order are indivisible and is devoted to the unfolding of personalities that know how to utilize, to fit into, and to sustain the subtly and intricately woven web of Hopi life. From birth to death the Hopi Indian is enmeshed in a complex system of interpersonal and intergroup relations, explicitly defined and fostered, yet yielding diversified, powerful, and stable personalities.

This burden of social ordering is mainly carried by two institutions—the

familial clans, which in turn are grouped together into larger systems or phratries, and the secret societies, mostly for the men and boys. The functions of these two institutions are diverse, though complementary. The clan is a widely ramified system of mutual and clearly defined interpersonal obligations and rights, drawing people together for mutual support through the powerful biological pull of kinship. It is within the family and the clan that children are rigorously trained from birth in the primal reciprocal human duties, rights, and skills—a habitual, conscious, defined ethical conditioning that leaves nothing to chance, yet ultimately relies on individual decision. The secret societies, on the other hand, initiate the successive generations of boys and young men into the higher ethical and intellectual preoccupations of the tribe. They are, among other things, the custodians and carriers of the ancient, elaborately beautiful, ritualistic ceremonies. These ceremonies embody the high esthetic, religious, and philosophical values of the tribe. Year after year, generation after generation, the Hopis devote an intense energy and a vast amount of time to the professional mastery and performance of these psychic dramas. Roughly speaking, the secret societies and the ceremonies are the institutions of higher education as contrasted with what might be called the primary education centered in the family and clan. And, by cutting across clanship lines, they counteract any divisive tendency among the clans.

Within this highly structured social framework the individual leads from birth a life that is oriented in minute detail toward a certain goal. That goal is the evocation of the individual's power and devotion for the benefit of the whole group and the conscious diffusion of his loyalties among the greatest number of people. At the heart of Hopi culture, therefore, is a thoroughly instrumented ethical principle which is almost diametrically opposite to the egocentricity deliberately cultivated by our acquisitive Western civilization. What kind of personality emerges from such a culture so oriented?

The method used by the authors in their study was to bring the various disciplines of anthropology, history, sociology, natural ecology, psychology, and psychiatry to bear simultaneously on determining how the total cultural complex, the nature-culture continuum, operates in shaping the individual personality. The result is a dynamic, integrated picture of the total Hopi culture from the economy to the total world-view. Treating the culture not as a static structure but as a dynamically balanced system of forces, the study then proceeds to its main quest: what kind of human being emerges from this particular system, and why?

A wide range of devices was used to get at the underlying personality structure of the Hopi people. There are detailed psychiatric case studies of many Hopi children, and a variety of psychological tests applied to large groups, up to the age of 18, designed to reveal intelligence, emotional responsiveness, moral ideology, and a detailed picture of the basic personalities of the children tested. The results of these studies are then correlated with, and interpreted from, the characteristic operating forces of the whole culture.

The individual case studies reveal two highly significant things. One is the wide diversity of personality among Hopi children—diversity of emotion, intellect, imagination, behavior. Indirectly, these results throw some light on the much-mooted question of the relative influence of environment and native endowment in shaping personality. Compared with ours, Hopi society is highly regimented; yet the stubborn inborn qualities of the unique individual per-

sist and come to fruition. A second finding is that personality maladjustments and warps arise in Hopi culture—and presumably in any culture—from the same types of environmental irregularities, from which we may infer that the primal demands of human nature are biologically embedded in the mind-body complex and are inescapable in any type of culture.

The various testing devices bring forth vividly the qualities of mind, imagination, and character of the Hopis, and above all the values by and for which they live. The intelligence of Hopi children up to 18 (the highest age level of the study) was found to be markedly higher than that of white children, as measured by identical tests. They especially excel in complex tests of the maze and puzzle variety. They reveal a capacity to weigh the significant details of complex situations and to deal with such situations as organized wholes. They have, in short, a highly developed capacity for "multidimensional" thinking. Unless one adopts the very improbable hypothesis of an innate intellectual superiority, one is forced, with the authors, to believe that the Hopi Indians have developed outstanding and whole minds because the entire fabric of their lives—from the precise fitting together of their rigorous agricultural techniques, through the intricately functioning clan and ceremonial structures, up to their cosmic philosophy of a reciprocally interacting universe—deals incessantly with complex but balanced organic systems.

The Hopi system of moral values is markedly different from our own. Personal ambition for the sake of the self, for prestige, is virtually unknown, and personal failure or inadequacy carries little or no weight. Individual motivations are directed outwardly toward the group, which probably accounts for the Hopi's extraordinary oversensitiveness to group criticism. Mere acquisition is not a Hopi incentive. Of all pleasure-giving activities, work with the group—in household or field or ceremonial—ranks among the highest. Aggression toward others is looked on with extreme aversion. The very name "Hopi" means "the peaceful people."

Yet the Hopi society is not a pure Utopia. The stress of the exacting physical environment and of the still more exacting social environment tells on the Hopi personality in a certain lack of spontaneity, often amounting to rigidity. A powerful, abstract, intellectual development has been gained at some loss of creative exuberance. Yet despite these stresses and strains, the Hopis are decidedly not a neurotic people. In fact, the Hopi personality is highly intelligent, balanced, rounded, stable, yet varied, richly developed, and frequently creative. It is also responsible, dutybound, cooperative. It is overwhelmingly group-minded rather than egominded. The Hopi has freedom—but freedom within the law. What he has sacrificed in emotional spontaneity he has gained in security and group survival. It is something more than blind love of ancient ways that makes the shrewd and sophisticated Hopis resist the encroachment of white civilization. From their lofty sky-cities, they look down on us in more than the physical sense.

The Hopis live richly, intensely, yet peacefully, and the substance of their lives is spiritual, not material. Has such a culture anything to offer to the modern distraught world beyond fortifying the yearning of many for a primitive escapism? I think it has many things, of which I shall mention only two or three. The Hopi society is an example of extreme decentralization. Not only skills but ethical sanctions are internalized in the individual to the point where the society can virtually dispense with coercive authority. The society is

self-managing. Modern government and industry, on the other hand, have steadily abstracted powers from individuals and primary communities and lodged them in central bureaucratic hierarchies. In this "managed" society, the anonymous standardized individual becomes a cog, or a serial number, with an undeveloped mind and personality suitable to his insignificant role. Such a society, in its later degenerative phases, as fascism shows, can be held together only by machine guns—and not very long.

The Hopi society is organized to develop complete, rounded, stable personalities devoted to the community rather than to the self. The Hopi people live intensely. Theirs is a pure democracy whose chief concern is the attainment of human excellence. In material things the Hopis are poor, but not squalid. They are not oppressed by material luxury or the need of it. Their wealth is people, not goods. The simple richness of their lives, the power of their minds, grow out of their preoccupation with the essential nature of things, with cosmic beauty and order, with the beauty of the earth and of the changing seasons and above all with the beauty of people and of human relations. The teleology of the cosmos, Whitehead has said, is devoted to the unfolding of variety, intensity, and beauty. In its own way, the Hopi culture exhibits these qualities in an organic community timelessly rooted in and flowering out of the order of nature.

Since the Renaissance, Western man has been dominated, in all aspects of his life, by the concept of egocentric individualism, fortified in more recent times by the doctrines of survival of the fittest and of automatic linear progress. These dominant Western concepts have played an important historic role in the development of science, technology, and our important but only half-realized ideals of democracy. Nevertheless, in their final stages, they have been primarily responsible for the disintegration of Western civilization, since they ignore the supreme nurturing function of society vis-à-vis the individual personality. Paradoxically, the frantic quest for egocentric individualism ended by more and more diminishing the individuality, the freedom, and the very souls of Western men. More and more they have been subjugated to the machines, the owners of machines, and the powerful centralized state whose *raison d'être* is to keep the machines running.

Western civilization has been darkened by the fog of an unworkable materialism which does not nourish the basic needs of the human personality. Its materialistic goals are juvenile, recessive, atavistic; the "abundant life" we visualized is trivial and thin. We have been building a pyramid civilization that has everything but a soul. It is therefore not surprising that our civilization has become predominantly psychoneurotic, and that modern men are filled with anxiety, boredom, cynicism, and hopelessness. Nor is it surprising that an economy devoted to egocentric materialism and human exploitation almost ceased to function except for war production. The extraordinary paradox of modern civilization is that, as men have gained unprecedented scientific power, they have steadily lost self-respect, pride, and dignity. Probably at no stage in history has the human ego been so dangerously deflated. It is not only disillusioned about itself, but it even has the temerity to be disillusioned about the cosmic process. These are not the normal symptoms of a healthy biological organism, nor is there anything visible in the nature of the cosmos to doom man perpetually to such a pathological outlook. They are the product of frustration. Our civilization is suffering from acute spiritual anemia.

No one would be so naive as to sup-

pose that a modern technological culture can adopt the ancient, prescientific culture of the Hopi Indians. But it is possible for modern civilization to create a rich, selfless, intense manner of social living, to reorient itself to the production of full human personalities, who in turn devote themselves cooperatively to great social ideals of beauty and excellence. The modern analogue of the Hopi democracy is a completely cooperative democracy, which will not tolerate the subjugation of man by man and which, by subduing the economic system to an efficient but secondary role, will open the way for developing an intense, localized, diversified, essentially spiritual culture. All men are potentially great because all men share in the universal mind. The essential goods of civilization are its constituent personalities. Human history does not move toward some "far-off divine" event. Fulfillment of life is here and now: "Our road is also our goal." The burden as well as the privilege of life is that it must be lived fully and intensely, each creature unfolding its full potencies and fulfilling its own nature. The alternative is degeneration.

The fateful choice of our civilization is not between guns and butter, but between half men and whole men. The Hopis cannot give us the blueprint for a new civilization, but they can instruct us in the nature of society as the nurturing ground of whole men and in the essence of true democracy, in which the eternal and yet infinitely malleable substance of human nature is wrought out to its full beauty.

SAINT LOUIS

From December 7, 1941 to August 14, 1945, scientists were preoccupied with war. Free neither to write papers nor to talk about their work, they had no time to travel to scientific meetings, few facilities for travel, and still fewer meetings to travel to.

Meanwhile new knowledge has been acquired; new applications of old facts have been discovered; new techniques have been evolved; new equipment has been invented. The scientific atmosphere is charged almost with the potential of the journalistically overworked atomic bomb, and it is appropriate to channel the pent-up energy into a scientific meeting. This is what the Association and many of its affiliated societies propose to do at Saint Louis from Wednesday March 27 through Saturday March 30, 1946.

The meetings are predestined to be a success. Perhaps there will be few definitive papers dealing with the phenomenal scientific developments of the war; possibly the exhibitors will have trouble filling their booths in the Auditorium with the latest gadgets and inventions, which are not yet in production; probably the overcrowded trains and hotels will cause personal discomfort, and the dates of the meeting—the only ones available anywhere for a meeting of this size—will prove inconvenient. But for other and more basic reasons the Saint Louis meetings will be a success: Scientists throughout the nation have much to talk over—perhaps more in the lobbies than in the meeting rooms. The isolation, the specialization, the pressure of the war years demand release through coversation and discussion.

Actually, the scientific fare, from A. J. Carlson's address of March 27 down to the smallest exhibitions in the Auditorium, gives every promise of being unusually good. But what guarantees success at Saint Louis is the resumption of scientific fellowship, the personal exchange of greetings, opinions, and experiences following the long and unrelieved years of war.—H. A. M.

THE DEVELOPMENT OF THE CONCEPT OF HEAT—I

FROM THE FIRE PRINCIPLE OF HERACLITUS THROUGH THE CALORIC THEORY OF JOSEPH BLACK

By MARTIN K. BARNETT

THE development of modern investigations on the phenomena of heat is connected with two distinct notions as to the nature of heat, the one asserting the substantial ("fluid") nature of heat, the other maintaining that what we perceive as heat is essentially a rapid vibration of the particles of the body which feels hot. It will be of interest to trace the history of these ideas, the roots of which are to be found in antiquity.

The Greek Notion of Fire. The phenomena of combustion must have early excited wonder and speculation, but the first European to give the concept of "fire" a prominent position in a system of natural philosophy was the Greek philosopher Heraclitus (cir. 500 B.C.). However, as Windelband (1, p. 36) notes, when Heraclitus declared fire "to be the essence of all things, he understood by this not a material or substance which survived all its transformations, but just the transforming process itself in its ever-darting, vibrating activity, the soaring up and vanishing which corresponds to the becoming and passing away." Hence, it would be a superficial view, indeed, which regarded the Fire Theory of Heraclitus as the root of the phlogiston theory or the caloric hypothesis which flourished over a thousand years later. Rather, in his conviction that all things are in a continual flux, that permanence is illusion due to the strife of opposites, he is closer to the kinetic view of heat.

A much closer approach to the substantial view of heat is found in Empedocles' (450 B.C.) four-element theory. He postulated as elements earth, water, air, and fire and thought that all change must consist in the dividing, intermingling, and separation of these fundamental constituents, regarded as imperishable, homogeneous, and unchangeable. The four-element theory gained great influence through its adoption by Plato (427–347 B.C.) and Aristotle (384–322 B.C.), the latter, however, introducing an important modification, namely, the postulate that the elements may be transmuted into one another by modification of their properties.[1]

Ellis (2, p. 76) thinks that earth, water, air, and fire of the Greek four-element theory correspond to the modern terms, solid, liquid, gas, and energy. With respect to the first three of these, we may agree with Ellis: all solids, all liquids, all gases were no doubt referred to as earth, water, and air, respectively, the generic terms not yet having been formulated. The identification of the Greek fire-element with energy is, however, objectionable. In the burning process, a bright fluid appears to emerge and it was no doubt this which Empedocles, and Aristotle after him, called "fire." On the other hand, the recog-

[1] Aristotle's properties were hot, cold, dry, and wet. Of these, hot and dry, especially hot, were supposed to belong to fire; hot and wet, especially wet, to air; cold and wet, especially cold, to water; and cold and dry, especially dry, to earth. By alteration of properties the one basic kind of matter could assume the form of fire, air, earth, or water.

nition of the peculiar nature of this "element" is already evidenced by the fact that Heraclitus chose fire as symbolic of the principle of *change* and restless activity. Aristotle (1, p. 147) regarded fire as endowed with a peculiar *centrifugal force*, and Democritus (460–360 B.C.) considered the "fire atoms" to be the cause of life processes.

Thus we see that in the Greek notion of "fire," three ideas representing different aspects of the combustion process are confused and they remained confused until modern times, when they finally received clear formulation in the distinct concepts of "quantity of matter," "quantity of heat" (or, more generally, "quantity of energy"), and "available energy" or "motive power."[2]

Greek Atomism. In the meantime, there had developed another view of the world which was destined to become, many centuries later, the foundation of the kinetic theory of heat. That view was Greek atomism. We have already noted how Heraclitus, captivated by the manifold change and variety in nature, had asserted that the essence of reality was simply flux or continual change, symbolized by "fire." On the other hand, Parmenides (470 B.C.), founder of the Eleatic School, adopting as his definition of Being, that which fills space (corporeality), reasoned that, since non-Being can neither be conceived of nor exist, therefore there exists only one Being which fills all space. Further, Being, if it were to change, could only pass into non-Being; because this is inadmissible, since non-Being does not exist, it follows that Being is eternal and unchangeable, and that all change as well as qualitative distinctions, must be illusory.

Democritus, the great systematizer of the Atomistic School, confronted with the opposition between the doctrines of Heraclitus and Parmenides, attempted to effect a compromise. He adopted Parmenides' notion of Being, as a stuff whose sole properties are eternality and corporeality (space-filling). But to explain the world of changing phenomena so insistently emphasized by Heraclitus, he supposed this Eleatic Being divided into small bits, scattered throughout empty space, and endowed with random motion. The continual flux of natural process, observed by Heraclitus, is nothing more, according to atomism, than the unceasing coming together and disintegration of the atoms, which, to be sure, are assumed to differ in size and shape. "All becoming, or change, is in its essence motion of atoms in space" (1, p. 43).

However, it is only insofar as the kinetic theory of heat may be regarded as derived from the general notions of atomism that Democritus and his followers may be regarded as the true fathers of the modern kinetic notions regarding the nature of heat. For when we inquire how the Greek atomists regarded the phenomena of fire and heat, we find that these were attributed not to any particular state of motion or configuration of the atoms of a body but to the presence in it of a particular species of atoms, namely, the "fire atoms." The "fire atoms," says Democritus, constitute a particular kind of atoms, namely, "the finest, smoothest, and most mobile." This phrase "most mobile" may be interpreted to mean that the "fire atoms" were characterized by a greater velocity than the other atoms. But the modern notion that an ordinary atom, by acquir-

[2] As an element in the same category as earth, water, and air, "fire" is "matter"; as identified with perpetual process (Heraclitus), it is "quantity of energy"; and as endowed with a peculiar force (Aristotle), as the "principle of disturbance" (Empedocles), as endowed with unusual mobility (Democritus), or as the principle of restless activity (Heraclitus), it corresponds roughly to "available energy," or "motive power."

ing through impacts a greater velocity, can become a "fire atom" is implicitly denied by Lucretius, the great popularizer of Greek atomism, when he states that atoms have always moved and always will move with the same velocity (3, p. 139).

The manner in which the Greek atomists regarded the phenomena of fire and heat is typical of the manner in which they explained many of the properties of matter: these properties were regarded as due to intrinsic, unchangeable properties of the atoms in question rather than to the momentary condition of these atoms. Thus the peculiar fluid nature of liquids was attributed to the smoothness and roundness of their atoms. Things which were painful and harsh to the senses were supposed to consist of hooked, rough atoms.

Thus, with respect to the nature of heat, Greek atomism is seen to occupy an intermediate position, which, indeed, was revived in modern times, especially by continental Europeans (4, p. 31). Insofar as all things were regarded as constituted of atoms moving in space, Greek atomism foreshadowed the kinetic theory of heat. But in postulating that fire and hot bodies contain *particular kinds* of atoms (although, to be sure, they were not considered to be atoms of an imponderable fluid), it tended to agree with the Aristotelian postulate of a fire-matter, which was the forerunner, not only of phlogiston, but also of a much more fertile conception, namely, Black's caloric.[3]

The Alchemical "Principles." The Hellenistic-Roman and Medieval Periods contributed relatively little to the heat concept. The best European minds of these centuries were occupied with questions of logic, ethics, or theological metaphysics. The only advances in the knowledge of heat were those gleaned by the alchemists of Egypt and Arabia and later of Europe, in their practical, partly magical, attempts to prepare gold from the baser elements and to discover the elixir of life.

Throughout this period, the teachings of Aristotle functioned as the guiding light, especially among the Arabians, who introduced them into Europe through the invasion of Spain in the eighth century and also through contacts made during the Crusades.[4] Like Aristotle they regarded the one fundamental matter as capable of appearing in four different elementary forms (fire, earth, air, and water) which could be changed one into another by a proper variation of properties. However, in their practical work with the metals, they came to regard Aristotle's particular choice of properties as inadequate. Thus we find Geber, an Arabian alchemist of the ninth century, introducing the properties ("principles"), "mercury" and "sulfur," the first to account for the luster, volatility, fusibility, and malleability of metals, the second to account for their color, combustibility, affinity, and hardness. To these Valentine added the principle "salt" which was supposed to remain intact on chemical treatment and was regarded as serving as a sort of base for the union of the principles, mercury and sulfur. By variation of these three properties, the metals were thought to be capable of transmutation into one another.

It is not difficult to understand how

[3] This statement is not intended to divert attention from the fundamental opposition between Greek atomism and Aristotelianism. For Democritus the only primary, i.e., real, properties of matter are extension in space and motion. Aristotle, on the other hand, was quite opposed to any attempt to reduce phenomena to mechanical causes, e.g., "hotness" itself was, for him, a primary quality.

[4] Of Aristotle's works, only a portion of his logic was transmitted directly to European posterity by means of the Christian theology. The remainder arrived much later via Arabia (1, p. 268).

the alchemists came to associate sulfur with combustibility. A mixture of sulfur and nitre was used as a fusion mixture for the metals, so that sulfur was thought to have the property (intense heat) of fire in this respect. Further, its yellow color suggested the bright color of "fire-matter," which appears to emerge from a substance, leaving an uncombustible residue.

We must realize that the terms "sulfur," "mercury," and "salt" were used, at least in the earlier centuries of the period, in a manner which, from our point of view, was metaphorical.[5] They denoted properties rather than substances, although this distinction seems gradually to have been obliterated.

System of Paracelsus. Thus we find Paracelsus (1493–1541) attempting to reconcile the Aristotelian four-element theory with the three principles of the alchemists by assuming that sulfur is the product of the action of fire on air; mercury, that of the action of air on sulfur; and salt, that of the action of water on mercury. Paracelsus was a man of great influence, and his system, however imaginative, reveals an important fact, namely, that the three principles had come, in the minds of the alchemists, to acquire a substantial character so that they were placed in the same category as Aristotle's four elements.[6]

What had happened was, no doubt, that the three-element theory, because of its value in explaining the behavior of the metals, had come more and more into the foreground with the consequent neglect of the Aristotelian doctrine, and that, as a result, the former finally usurped the province of the latter, at least in the field of metallurgy, so that the three principles came to be regarded, not only as properties, but as substances as well. Then the system of Paracelsus may be regarded simply as an attempt to justify current alchemical ideas by referring them back to Aristotelian doctrine, regarded as fundamental.

That by the latter part of the seventeenth century, the sulfur principle, in particular, had come to acquire a definitely substantial character and, indeed, to be more or less identified with the substance which we now denote by that name, is evident from Becher's (1635–82) criticism of the alchemical combustion theory. Sulfur, he says, itself burns and therefore cannot be the principle of combustibility but must, like the metals, contain it. Becher definitely postulates that combustion is a process of decomposition, a view which seems to have been held by all of Aristotle's followers, and states that in the burning of a substance the combustible principle, which he calls *terra pinguis* (fatty earth), is evolved.

Advent of Phlogiston. Stahl (1630–1734) substituted the term "phlogiston" for Becher's *terra pinguis* and skillfully employed the hypothesis, under the name of the "phlogiston theory," to correlate the phenomena of calcination, combustion, reduction, and metallic displacement of hydrogen from acids.

Although the phlogiston theory came to exert a commanding influence on most of the chemists of the eighteenth century, it nevertheless shared the fundamental weakness of the sulfur theory which had preceded it, namely, the confusion between property and substance. Nor was this alleviated by Stahl's statement that phlogiston is "the material and principle of fire, not fire itself" (5).

The increase in weight of a substance, on burning, and the essential part played

[5] Wisely avoiding confusion, Brown (5) calls the alchemical principles "*philosophical* sulfur," "*philosophical* mercury," etc.

[6] This is particularly evident from Paracelsus' description of the burning process: "That which smokes and evaporates over the fire is mercury, that which flames and is burnt is sulfur, and all else is salt" (2, p. 161).

by the surrounding air were the main facts which finally overthrew the phlogiston theory of combustion. The advocates of the theory met the first objection in two ways. Some of them regarded phlogiston, not merely as an imponderable, but as actually endowed with negative gravity: they regarded its presence as functioning to decrease the effective weight of a body. This idea was advocated by numerous alchemists before Becher's time and indeed was shared by Aristotle who, as we have noted, endowed his fire-matter with a peculiar centrifugal force. Others maintained that the increase of weight was due to the addition of *ponderable* fire-matter during the combustion process: the fire in the flame was thought to penetrate the container to which it was applied and combine with the burning substance. The second objection, that the presence of air was necessary for combustion, was met by the assumption that it (the air) functioned as an absorbing agent for the phlogiston liberated.

By the latter part of the eighteenth century, the phlogiston theory had become extremely unwieldy; in fact, phlogiston had been variously identified as light, electricity, soot, and hydrogen, and indeed, in France, was still associated with sulfur (5). Hence the penetrating researches of Lavoisier (1775) on the weight relations involved in combustion were quite sufficient to cast out from the minds of most chemists the 2,000-year old notion that burning is a process of decomposition and to establish in its place the postulate that combustion consists in the union of the burning substance with the oxygen of the atmosphere.

However, as Merz (6, Chap. VIII) has noted, the phlogiston theory, insofar as it asserted that something was liberated in combustion, contained an element of truth which was destined to bear fruit. Lavoisier's great service showed that the mysterious phlogiston had no place in a description of the *weight* changes incurred by burning. But a more careful investigation of the changes occurring in the surroundings was to lead to a sort of reincarnation of the phlogiston theory. Our modern phraseology still bears witnes to that reincarnation, for we say that heat is liberated in burning.

Nor had the concept of an imponderable fluid, exemplified by phlogiston, reached the end of its scientific usefulness. Rather, this end had come only in the case of chemistry proper, which, following the example of Lavoisier, proceeded to concentrate on the study of the *weight* relationships in chemical change. In those branches of physics concerned with changes of state or condition, the concept of an imponderable fluid continued to be of service. We shall see that, as caloric, it rendered invaluable service in comprehending and classifying the phenomena of change of state, expansion, compression, and reciprocal changes of temperature. In the eighteenth century Watson (1746) and Franklin (1706–90) independently proposed fluid theories of electricity. Later, Coulomb (1785–1789) advocated the two-fluid theory to correspond to positive and negative electricity. He also proposed two magnetic fluids. These fluid theories of electricity and magnetism served as the basis for the mathematical formulation of the laws in these branches of physics.

Rise of the Kinetic Theory of Heat. In the meantime, epoch-making advances in astronomy and mechanics had brought the motion concept into great scientific prominence, and this fact, together with the revival of Greek atomism, led more imaginative minds to the formulation of the modern *kinetic* theory of heat.

It is of interest to note that in the cases of Bacon (1620) and Descartes (1640), motion theories of heat are advanced independently of atomism. This was

done, in the crudest form, by Francis Bacon (7, Chap. VIII) who, impressed by the motion of boiling liquids and fire, asserted heat to be the outward, upward, expansive motion of the small parts of a body. His doctrine is not a revival of atomism, for he states, specifically, that the motion is "not in the very minutest particles but in those of some tolerable dimensions," nor does he postulate a void. He does not explain how a liquid can be in violent motion without being hot or how, on the other hand, a red-hot iron may be in a state of rest. Descartes, though denying the existence of a void and postulating the infinite divisibility of matter, nevertheless states motion to be the cause of all change. In particular, he regards the sensation of heat as due to motion communicated to the nerves, an old idea of Democritus (1, p. 114).

Greek atomism was revived and popularized through the efforts of Magnenus (1646) and especially Gassendi (1647). The union of the motion and atomic concepts to account for thermal phenomena had not long to wait, for we find Boyle (1629–91) asserting, in quite the modern fashion, that heat is a molecular motion and Locke (1690) describing it as "a very brisk agitation of the insensible parts" of an object (8, p. 24). Hooke (1665) adopted a similar view toward the property of fluidity, attributing this, not to the shape of the atoms of the fluid, as the Epicureans had done, but to "a certain pulse or shake of heat."[7]

Newton, too, who as Merz (6, Chap. VI) notes, first made the kinetic view of nature scientifically possible through the publication of his *Principia* (1687), states that "heat consists in a minute vibratory motion of the particles of bodies" (9, p. 84). These views were strengthened when Bernoulli (1738) laid the foundation of the kinetic theory of gases.

Relation of Light and Heat. The tendency to associate heat with motion received a fresh impulse from the establishment, by experiment, of the close relationship existing between light and radiant heat. For whether light be viewed according to the corpuscular emission theory favored by Newton or the undulatory theory of Huygens (1690), it is, in either case, a motion (in the first case, a streaming motion of particles; in the second, a vibratory motion in the ether), so that radiant heat, so closely related to it, must also be of the nature of motion. We have just noted Newton's kinetic view of heat. Huygens also held such a view, for he not only states that light is a motion, but also that the solvent properties of heat and flame, the causes of light, indicate that they too are of the nature of motion (10, p. 211).

The close connection between light and heat had already been vividly exhibited by the properties of the burning-glass of Archimedes (287–212 B.C.), and in the thought of the medieval alchemists the two were pretty thoroughly confused. In modern times we find a renewed interest in these phenomena. Tschirnhausen (1699), concentrating the rays of the sun with two large lenses, succeeded in burning wood, boiling water, and melting lead and iron. By coating the metals with charcoal, he was able to volatilize them, from which he deduced the salient conclusion that black bodies absorb the most heat. At the same time he cast doubt on the legitimacy of generally identifying light and heat when he observed that the rays of moonlight could not be made to produce any noticeable heating effect.

Boyle, by noting that the burning-glass remains operative in a vacuum, had already demonstrated that the propaga-

[7] "For heat being nothing else but a very brisk and vehement agitation of the parts of a body," these "are thereby made so loose from one another, that they easily move any way and become fluid" (2, p. 96).

tion of heat and light is independent of the material medium, and Scheele (1777) verified this with the observation that heat felt ten feet from an oven is unaffected by an intervening stream of air. Scheele also made the important distinction between this sort of heat which he called "radiant heat" (*strahlende Wärme*), and the heat passing out through the chimney with the smoke (convected heat).

Lambert (1779), no doubt influenced by the emission theory of light, assumed that radiant heat consists of streams of "fire-particles," reminiscent of Democritus. He was also convinced that these "fire rays" obeyed the same laws as light, and proceeded to deduce the laws of the burning lens from the principles of optics. At the same time, he was aware that the rays of "dark heat" (infrared rays) were reflected in the same manner as light.

The close similarity of light and radiant heat was further demonstrated by Pictet (1790), who noted that a thermometer placed at the focus, or "burning point," of a concave mirror immediately registered a rise in temperature when a hot body was placed at the focus of a second mirror, coaxial with the first but at a long distance from it. From this he concluded that radiant heat must be propagated in straight lines at a very great velocity, perhaps at the velocity of light itself. He contrasted this with conducted heat which, unlike radiant heat, must proceed from particle to particle of the medium and consequently travels much more slowly. However, a complete distinction between radiant and conducted heat is not attained by Pictet, for, on failing to concentrate the heat from boiling water by means of a glass mirror, he thinks this might be done with a mirror which is a better conductor of heat, e.g., a metal mirror.

Hutton (1794) identifies light with radiant heat and postulates that dark bodies also emit radiation, even though undetectable by the eye. A hot body, he says, converts heat to light which, on absorption, becomes heat again. A step backwards appears to have been made by Herschel (1800) when he stated that every ray consists of a light ray and a heat ray.

Rumford (1805), famous for his experiments on frictional heat, regarded a body which radiates heat as analogous to a bell sending out vibrations (heat waves). The temperature of the hot body, he says, is determined by its period of vibration. A series of experiments led Rumford to further important conclusions: (1) all bodies radiate at every temperature; (2) the intensity of radiation is different at the same temperature for different bodies (substances); (3) bodies at the same temperature do not influence each other through radiation.

To Leslie (1804) is due the important observation that strongly reflecting surfaces are also poor emitters of radiation and that heat absorption and heat emission increase and decrease together. He also discovered that the intensity of radiation (from a unit area of surface) at a point outside a radiating body decreases with the angle made by the surface of the body with the line of propagation of radiation to the point. Leslie's speculations on the nature of heat are less fortunate. For, ignoring Boyle's discovery that radiant heat is propagated in a vacuum, he attributed radiant heat to air pulsations set up by the radiating (vibrating) body, thus confusing radiation with convection.

Prevost (1809), who clearly distinguished between the radiation and conduction of heat, adopted the emission theory of radiant heat (*calorique rayonnant*) and developed it along lines suggested by Bernoulli's kinetic theory of gases. He notes that glass retains "dark heat" but allows radiant heat to pass through and thinks this may be due to

the fact that there are several kinds of heat particles. Rumford had already postulated that all bodies radiate at every temperature,[8] and Prevost brought this principle into great prominence in his celebrated "Theory of Exchanges." A hot body is simply one which emits heat particles at a greater rate than colder bodies. The apparent state of rest characterized by equality of temperatures is really, says Prevost, a state of dynamic equilibrium in which the bodies emit the particles of "radiant caloric" at the same rate.

It will be unnecessary to describe here Fourier's (1817) mathematical systematization of the facts of radiation, nor shall we relate the long series of investigations, extending through the nineteenth century, which served to establish the agreement of radiant heat and light in all their properties, with the resultant triumph of the undulatory theory of Huyghens, the latter having been revived and developed by Young (1801) and Fresnel (1819). Rather, it will suffice, for our purposes, to point out just what significance is to be attached to that period of the history of the subject, ending in the early years of the nineteenth century.

In the first place, we cannot ignore the heuristic significance which these investigations had for the establishment of the First Law of Thermodynamics. For, entirely independent of the question of the precise nature of light and heat, the facts of their close association, conformity to the same laws, and their convertibility constituted undeniable evidence in support of that comprehensive, though vague, view that all of the "forces" of nature are "correlated." The precise manner in which this notion of "correlation of forces" contributed to the founding of the Energy Principle naturally belongs to a history of that principle.

Of more immediate concern is the effect of these radiation studies on current ideas concerning the nature of heat. We have already remarked that, insofar as radiant heat, like light, is regarded as a motion of something, the study of it tended to bolster up the kinetic theory of heat. If, nevertheless, the caloric or fluid theory continued to maintain its supremacy throughout the eighteenth century, this may be attributed, to a large extent, to the circumstance that Newton's emission theory, not Huyghens' wave theory, of light (hence of radiant heat) prevailed among most of the physicists of that period.[9] For if light is to be regarded as a rectilinear motion of very fine particles, radiant heat must also be so regarded. And, since the radiating body exhibits no loss in weight, the particles must be those of an imponderable fluid, naturally identified as heat fluid, or caloric, since the radiating body suffered a fall in temperature.[10]

[8] Hutton first noted the contradiction involved in assuming that only the hot body radiates: the same body, at the same temperature and in exactly the same state, is assumed to radiate, or not radiate, depending on whether it is the hotter or the colder of the two bodies.

[9] This we may attribute to the failure of Huyghens' theory to account for the nature of shadows and for rectilinear propagation, together with the fact that Newton's great authority opposed it. Young (1800) revived and expanded the wave theory in England, but his work was hardly noticed. It was Fresnel (1817), working in France, who finally convinced scientific authority of the superiority of the undulatory theory, but only after overcoming vigorous opposition. Thus his most important memoir on the subject, though submitted to the French Academy in 1821, was not published until 1827 (11, ii, p. 115).

[10] A curious dilemma must have presented itself to those scientists who opposed, on general principles, the assumption of an imponderable fluid. For if the existence of an imponderable heat fluid be denied, that of another, namely, Huyghens' ether, must be admitted to account for the propagation of radiation through a vacuum.

(To be concluded)

SCIENCE ON THE MARCH

THE SPECIFIC EFFECTS OF CERTAIN TEMPERATURES ON STORED FRUITS, VEGETABLES, AND FLOWER BULBS

For many years it has been the custom to hold certain fresh fruits and vegetables in cold storage for the purpose of preventing spoilage. It is the general consensus that the low temperatures in the storage rooms serve to arrest or retard the biological processes that bring about senescence and decay, and for the most part this belief is correct. However, there are certain physiological processes that not only are able to continue at these low temperatures but even appear to be accelerated. Sometimes a certain temperature or narrow range of temperatures appears very specific in producing certain effects on fruits, vegetables, tubers, and bulbs.

It often happens, for example, that when citrus fruits are held at 32° F. for four to six weeks there develops a physiological disorder known as "watery breakdown." Although it gives the fruits the appearance of having been frozen, carefully controlled experiments have demonstrated that this disorder may be produced by low temperatures that are still above the freezing point of the fruits. Pitting (pox, storage spot) is another low-temperature disorder of citrus fruits. It is characterized by shallow, pocklike depressions in the rind, which become discolored in the advanced stages. Temperatures most conducive to pitting are 32° for lemons, 36° to 40° for oranges, and 40° for grapefruit.

Still other, more or less superficial, rind blemishes of citrus fruits are "scald" of oranges and grapefruit and "peteca" and "red blotch" of lemons. "Membranous stain" of lemons, a darkening of the membranes between the segments, has been found much worse at 36° and 40° F. than at 32°, 50°, and 60°.

Tropical fruits are still more sensitive to low temperatures than are the subtropical citrus fruits. If papayas are held for only 5 days at 45° F. or lower, they become chilled, and the treatment so upsets their metabolic processes that they will not ripen properly when removed to higher temperatures. Experiments have shown that certain chemical processes that accompany normal ripening, such as inversion of sucrose, are arrested by exposure to low temperatures and are not resumed when the fruit is brought back to room temperatures. In like manner avocados, pineapples, and bananas experience a physiological breakdown when held too long at low temperatures. The result is usually a darkening of some of the tissues when the fruit is removed to higher temperatures. It is not necessary to drop very low in the temperature scale to produce deleterious effects in bananas. Green fruits, if held at temperatures below 56° for four or five days, will subsequently fail to ripen properly in the ripening rooms. Bananas that have been chilled after ripening will develop a dull-brown color when later exposed to higher temperatures, and are very susceptible to handling marks, the slightest bruising causing discoloration. Darkened bananas sometimes appear on the fruit stands after the merchant or the deliveryman has been caught unawares by sudden and unexpected cold weather, and the consumer has been known to hold bananas in the refrigerator long enough to have them darken when brought out into the room.

Tropical vegetables may suffer from low-temperature injuries just as much as tropical fruits. Tomatoes are not

ordinarily thought of as tropical because they are grown so extensively in temperate latitudes, yet their home is in the tropics. When wholesalers have attempted to hold too long at near 32° the mature green tomatoes, or "green wraps," as they are commonly called, they have experienced rather disastrous results. These chilled tomatoes, when removed to the ripening rooms, will ripen with a dull-yellow color, or they may decay before they ripen.

The average farm boy knows that the first frost is the signal to dig and cure sweet potatoes before they are injured by cold. Darkening and internal breakdown of the tissues occur in storage rooms also, when the temperature is below 40° F. The susceptibility of sweet potatoes to low-temperature injury is reduced by proper curing. It is interesting to note that some success in prestorage "curing" of grapefruit has been reported, the processes for the two products being alike in principle, though not in details.

Even flower bulbs are subject to specific physiological effects of low temperatures. Several years ago in experiments in the U. S. Department of Agriculture, King Alfred narcissus bulbs were held during three successive storage periods. Temperatures during the first and third periods were in the range of 60° to 90°, but the middle period came within the range of cold storage, that is, 32°, 40°, and 50°. When these bulbs were held at 40° during the middle period of storage there subsequently developed more "blind," or nonblooming, bulbs than when they were held at 32° or 50°.

Here is an instance of the specific effect of one temperature, or perhaps a narrow range of temperatures. Another example, previously mentioned, was that of 36° and 40° in their relation to the development of membranous stain in lemons. Other effects of specific temperatures might be cited. Continuous storage at 50° F. produces "pumpkin-yellow" grapefruit, a good red color in Haden mangoes, and considerably more "blood" spots in the so-called blood oranges. These effects are not produced by temperatures some degrees higher or lower, or at least not so rapidly.

Examples can be cited in which certain desired physiological changes have been produced in fruits, vegetables, or flower bulbs by employing the proper temperatures in storage. Early-season Bartlett pears from California arriving on the Eastern markets during July and August frequently fail to ripen with good color, flavor, and texture. Tests made by the U. S. Department of Agriculture showed that if these pears were ripened at 65° to 70° F. the flavor, color, and texture were satisfactory. Those ripened at 80–85° F., the temperatures prevailing in the stores at the time, were of poor quality. Fall and winter varieties of pears likewise are of best quality when ripened at temperatures between 60° and 70° F. Kieffer pears are usually considered to be of very low quality, and it is probable that this reputation has been based on the fact that they usually ripen during exposure to too high temperatures, at least in the South. Experiments have shown that when this variety of pear is ripened at 55–65° the quality is greatly improved, although it never becomes the equal of such a variety as Bartlett.

About 1928 or 1929 several restaurant operators and manufacturers of potato chips came to the U. S. Department of Agriculture with a problem. They stated that their French fried potatoes and potato chips were too dark for the customers and that the dark chips had an undesirable flavor. Now a dark color in a cooked product may be due to the caramelization of sugar, and the Government research men knew that there are certain storage temperatures at which sugar accumulates in Irish potatoes.

This had been worked out many years ago at the University of Maryland when it was reported that Irish potatoes become sweeter if held in storage at 40° F. or lower. A checkup revealed that the potatoes used for French "fries" and chips in this case had been previously stored at 32°. Subsequent tests by the U. S. Bureau of Plant Industry in cooperation with the Bureau of Home Economics of the U. S. Department of Agriculture revealed that if potatoes were stored at 50–60° F. for a while immediately before use they could be made into chips or French "fries" white in color and of good quality.

Examples may also be cited in which physiologists have utilized low temperatures for the purpose of stimulating seed production in certain vegetables. For instance, the production of flowers by sugar beet plants is objectionable when these plants are being grown for sugar, but when the object is the production of seed, the development of flowers is quite obviously necessary. The initiation of seedstalks and the flowering of biennial beets have been shown to be due to the cumulative effect of low-temperature exposure followed or accompanied by the effect of long photoperiods (duration of daylight). When research workers in the U. S. Department of Agriculture were breeding sugar beets and growing them in that part of the country where the length of day was insufficient to induce flowering, they overcame this by the use of cold storage. The tendency to "bolt," or produce seedstalks, was greatly stimulated by exposing the beets to low temperatures (33–38° F.) before planting in the field. The treatment could be applied to the "mother" beet roots or to the germinating seeds.

Similar work has been done on onion bulbs. Storing them at 53.5° F. for four months prior to planting has caused the plants to blossom and ripen earlier and to yield more seed per acre.

One more story of the specific effects of certain temperatures might be cited. Bulbs of the Creole type of Easter lily, when planted immediately after midsummer digging, bloom in the period between April 3 and April 12. This lily is in great demand at Easter. However, Easter is one of the few holidays that has no definite calendar date. It may come any time from late in March until late in April, and the disastrous results for the florist of having his lilies bloom too early or too late can easily be imagined. There is still another reason for desiring to change the blooming habits of the Creole lily. In some sections of the country the trade demands lilies at Christmas. To meet these varying demands for lilies, research workers attempted to modify the physiological response of lily bulbs by storing them at different temperatures following harvest. In these experiments the unstored lily bulbs bloomed 259 days after planting. However, if stored 10 weeks at 36°, 40°, 45°, 50°, 55°, and 59° F. after the bulbs had matured, blooming occurred from 111 to 135 days after planting. The temperature treatments that hastened the blooming of these bulbs also tended to reduce the number of blooms per plant, and this effect increased with increase in storage temperature. Because of this fact, 36° to 45° F. is usually recommended for storage of Creole lily bulbs intended for forcing of bloom.

The lay reader often objects to the manner in which scientific material is presented, pointing out that the impartial viewpoint usually leaves the reader with no definite answer to his questions. On occasions when this subject has been presented to groups or organizations, the speaker is invariably questioned as to the proper, rather than improper, temperatures at which various products should be stored. This information is available in publications of the State and Federal agricultural research agencies. For

some of the fruits and vegetables mentioned above the U. S. Department of Agriculture makes the following recommendations for temperature of storage: Bananas, ripening, 62° to 70° F., holding ripe fruit, 56° to 60°; grapefruit, 32° to 34° if grown in regions where decay is a serious factor, or 45° to 55° if from other regions; lemons, 55° to 58°; oranges, 34° to 38°; pineapples, mature green, 50° to 60°, ripe, 40° to 45°; potatoes, no lower than 40° for table or seed stock, unless the seed is to be kept longer than 3 to 5 months, in which case 36–38° F. is recommended; sweet potatoes, 55° to 60°; tomatoes, ripe, 40° to 50°, mature green, 55° to 70°.

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FACTORS CONTROLLING THE CONCENTRATION OF ERYTHROCYTES IN THE BLOOD*

THE existence of a sex difference in the erythrocyte (red cell) concentration of the blood has been observed in numerous animals, among which are the domestic fowl, pigeon, ring dove, mouse, rat, rabbit, cat, dog, sheep, horse, and man. In each of these species the erythrocyte count of the male is consistently higher than that of the female. For example, in the domestic fowl the average count for males is 3,260,000 red blood cells per cubic millimeter of blood, while that of females is 2,660,000. Similarly, in humans the average red cell count for a man is approximately 5,000,000 as compared with 4,500,000 for a woman.

It has been shown by observations on many castrated animals that this difference between the sexes is due, at least in part, to the reproductive glands: ovaries or testes. In a male such an operation is followed by a fall in the erythrocyte count, while in a completely ovariectomized female there is either no significant change or there is a slight rise in the number of red blood cells. Therefore, the castrated male and the ovariectomized female have approximately the same red cell concentration. Furthermore, if a testis is successfully grafted into such a castrated rooster, the red cell count will rise again to the normal male level.

In the domestic fowl, as well as in most other birds, there is only one functional ovary—the left one, for the right gonad remains undeveloped and persists throughout life as a nonfunctional rudiment. If the functional left ovary is removed from a young chick, the right rudimentary gonad will develop into a testis-like organ which has the endocrine characteristics of both an ovary and a testis. A bird possessing such a gonad frequently shows both male and female characters, such as the large comb of the male and the henny plumage of the female. It is also interesting to note that the erythrocyte counts of such individuals lie somewhere between the normal male and female levels but are usually closer to that of the male.

When male sex hormones (androgens) are injected or administered by means of pellets implanted beneath the skin, the number of red cells will increase in the castrated male or female bird, rat, dog, monkey, or human, while treatment with female sex hormones (estrogens) will induce anemia, or a decrease in the number of red corpuscles. By means of such androgen pellets the red cell concentration has been maintained at the male level in groups of castrated roosters, or capons, for periods of over two years. When pellets are almost or completely absorbed, the erythrocyte count drops, but after each subsequent implantation it will rise again to the male level.

* This investigation was aided by a grant from the Dr. Wallace C. and Clara A. Abbott Memorial Fund of The University of Chicago.

Since sinistrally ovariectomized poulards (hens from which only the left ovary has been removed) develop right testis-like gonads secreting both androgens and estrogens and usually show a red cell count intermediate between the male and female, it became of interest to see if such a condition could be produced experimentally in normal or castrated animals by simultaneous androgen and estrogen treatment. This problem has been approached in two ways:

(1) On the fourth day of incubation (the normal incubation time for the domestic fowl is twenty-one days) before the reproductive glands have developed in the embryo, a small amount of estrogen was injected into the egg, which was then allowed to continue incubation and to hatch. Genetically, male chicks hatched from such treated eggs are of an intersexual type when mature, possessing gonads which secrete both androgens and estrogens, as do those of sinistrally ovariectomized poulards. Such individuals also have erythrocyte counts intermediate between those of normal males and females.

(2) Capons were injected with, or received pellets of, both male and female sex hormones simultaneously. If the proportion of estrogen administered is low as compared with the androgen, the red cell counts rise to the male level. If, on the other hand, the estrogen dosage is relatively high, the counts remain unchanged or fall. Thus far a duplication of the situation found in sinistrally ovariectomized poulards and in intersexual males which received estrogen treatment during incubation—where the red blood cell count was intermediate between those of the male and female—has not been observed. This may be due to an improper balance between the concentrations of the two sex hormones administered, or it may be that the methods employed in administering the hormones do not duplicate, in fully grown birds, the condition prevailing in poulards and intersexual males, where the influence of mixed male and female hormones is not only exerted relatively early in development but is also regulated by the bird's own endocrine system.

Also of interest in connection with the problems arising from simultaneous androgen and estrogen administration are observations which have been made on laying hens. Although the domestic fowl is not a seasonal breeder, as are most wild birds, there is, nevertheless, in the average flock, an increase in egg production in the late spring and early summer. During the laying period there is evidence of an increase in both estrogen and androgen production by the hen's own ovary. In spite of the increased amount of androgen, the erythrocyte counts made during the spring laying periods are lower than at any other season of the year. This would seem to indicate one of three possible interpretations: (1) the increase in androgen is not of sufficient magnitude to raise the erythrocyte level; (2) the increase in estrogen is sufficient to lower the red cell count regardless of the simultaneously increased androgen; (3) the seasonal cycle in females is caused by factors other than male and female sex hormones.

Of particular interest at the present time are certain observations made on male and female human blood donors. There have been several recent reports indicating that red cell regeneration after blood donation is slower in women than in men. Similar observations have also been made on rats in which severe hemorrhages were induced. It was found that androgen injections stimulated, while estrogen treatment actually inhibited, the regeneration of erythrocytes. This would seem to indicate that in females regeneration of red cells is normally inhibited by the animal's own sex hormones, whereas in males regeneration is accelerated.

The red blood cells are produced in the marrow cavities of the long bones and are destroyed and removed from circulation largely by the liver. The increase in the number of red cells in androgen-treated animals, therefore, could be due either to a stimulation of their production or to a decrease in the rate of their destruction. Conversely, the lower count in animals treated with estrogens might be due to an inhibition of erythrocyte production or to an increase in their rate of destruction.

Studies on the bone marrow of pigeons, mice, and rats, made chiefly by Gordon and his co-workers at New York University, have produced evidence that the first suggestion may be correct. These observations have shown that estrogens cause the formation of bone within the marrow cavities, thereby interfering with the production of red blood cells, while androgens appear to stimulate the erythrocyte-producing (erythropoietic) tissue within the marrow to greater activity.

It must not be concluded that the male and female sex hormones are the only agents which affect the concentration of the circulating erythrocytes. For many years it has been known that oxygen deficiency stimulates the bone marrow to produce more red cells, resulting in higher counts in individuals living at high altitudes. Also, it has been known that in fevers, as well as after severe muscular exercise, there may be a marked increase in the number of red cells (polycythemia). Likewise, the administration of cobaltous salts in some animals causes a marked polycythemia, which, in extreme cases, is fatal. On the other hand, various poisons, such as lead, may injure or destroy the erythrocyte-forming tissues, and the lack of certain substances in the diet, such as an adequate amount of iron, will cause an anemia. Likewise, other endocrine glands, notably the thyroids, adrenals, and pituitary, have been found to play a part in the regulation of the red-cell concentration.

Thyroid insufficiency will bring about a drastic reduction in the number of red blood cells. This has been interpreted as due, not to the direct effect of inadequate thyroid hormone upon the bone marrow, but rather to a more general metabolic effect, causing a slowing down of most of the physiological activities of the body. An overabundance of thyroid hormone produces a high erythrocyte count by stimulating metabolic activity, thereby creating an oxygen deficiency which causes an increase in the production of red blood cells.

Adrenal insufficiency results in a spectacular rise in the number of red cells, while treatment with extracts from the adrenal cortex reduces the number within a few hours. This is not believed to be due to any actual increase or decrease in the number of erythrocytes in circulation, but rather to an increase or decrease in the fluid constituent of the blood, which in turn would affect the apparent number of red cells.

The pituitary gland itself operates indirectly as a regulating mechanism since it produces hormones which control the activity of the reproductive glands, thyroids, and adrenals. Some investigators have postulated a specific pituitary hormone which has a more direct control over the concentration of red cells, but this hypothesis has few adherents.

Since all of these regulating factors, with the exception of the ovarian and testicular hormones, are presumably operating similarly in both males and females, it would seem plausible to conclude that the sex hormones play the major role in bringing about the differences observed in the erythrocyte counts of males and females.

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BOOK REVIEWS

THEORETICAL ASPECTS OF HISTORY

The Philosophy of American History. Morris Zucker. 2 vols. Vol. I. *The Historical Field Theory,* 694 pp. Vol. II. *Periods in American History,* 1070 pp. 1945. Arnold-Howard Publishing Company, Inc., New York.

THE first volume of this work claims to lay the theoretical foundations for a science of history. The second volume purports to apply the "historical principles" developed in the first volume. The present review will concern itself mainly with the first volume, as the second is devoted entirely to the familiar events of American history. As history, Volume II should be left to historians to evaluate. Both volumes abound in diatribes against the injustices and the imbecilities of our times.

There is no question about these maladjustments or the vigor with which the author expounds them. Indeed, his criticism and ridicule of most of the historians and the "statesmen" of the day are by far the most effective and valid part of his work. I share most of the author's prejudices on these matters and consequently had an inordinately good time reading these volumes. As an argument for the application of science to social affairs and an exposé of the disasters resulting from not doing so, the two volumes are sound, witty, and highly readable. As a treatise of scientific method or an example of its application, the work unfortunately leaves much—nearly everything—to be desired.

In spite of all his lip service to science in general and to field theory in particular, the author finds it necessary to repudiate the post-Einsteinian developments of that theory, which he finds inapplicable to social science. Instead he finds it necessary to adopt a view of causality (p. 585), which has been pretty well abandoned in modern philosophy as well as in science. The section dealing with these matters is thoroughly out-of-date. A few examples must suffice.

In insisting that the social sciences must rely on a type of causality abandoned in the other sciences, the author betrays a basic misunderstanding regarding the nature of all scientific generalizations whatsoever. The "inherent fallacy of the statistical method of analyzing vital phenomena" is illustrated, he thinks, by the following example:

> It can be shown that the average life expectancy is today much greater than it was twenty, fifty and a hundred years ago in every age class. The reasons for this cannot be discerned by the minutest scrutiny of the actuarial tables. They will not show the causes, but merely record the results of a long process of development within society which manifested itself in the standards of living, general sanitation, purer food, the growth of public medicine and a myriad of other factors which no statistical table will disclose. These can be established only by the patient labor of the classical scientific method (p. 584).

One wonders, in the first place, how, except through statistical methods, the manifestations of improved standards of living, general sanitation, etc., is revealed at all. Whatever *scientific* knowledge we have of these phenomena, *as well as of the processes by which they have transpired,* has been achieved through the correlation of these data and "myriads of other factors" in the testing of hypotheses according to rules recognized in all science. Correlation of masses of data can be *reliably* carried out only through statistical methods. It is precisely here that scientific methods differ from those of Mr. Zucker and other historians. Historians also engage in correlation—informally, usually of cases selected to fit the theory with which they start, with no inconvenient questions of sampling or probability to mar the author's literary style. As such, their work provides valuable hypotheses for science. To regard these conclusions as more than

hypotheses is to confuse hypotheses and laws.

I invite any scientist to scrutinize the 1700 pages of this work to see if he can find a single proposition that he recognizes as a scientific law of the kind he credits in the other sciences. The reader may also meditate on Mr. Zucker's understanding of the nature and requirements of scientific generalizations as revealed in the following statement:

> There has never yet been a graph scientifically constructed which forecast exactly [*sic*] the number of deaths for a certain year. If guesses have been correct, they have been nothing more than lucky guesses (p. 585).

In short, we have in these volumes another attempted short cut to social science. The model and the method are those of Marx. Admirable as these may be in their own way they differ sharply from those of science. The author has himself inadvertently suggested one of the main differences in the following passage:

> In any event, a great deal depends upon whether we approach its [history's] investigation from the standpoint of causal or statistical law. If looked at from the statistical point of view, it is all a matter of chance, accident, probability, despite all we can do. If considered from the causal standpoint, then no doom is inevitable so long as man exercises some rational power over the unfoldment of the historical processes (pp. 586–587).

In science, and especially in field theory, the "rational power" of man *is itself part of the field and is as subject to the laws governing the field as any other element.* It is precisely this fact that many historians and some who pass for social scientists are not yet prepared to face. Until they are prepared to do so, they had better be modest about their scientific pretensions, especially so far as drawing on the theories of modern physics are concerned.

I am in profound agreement with the author's enthusiasm for the application of science to human affairs and in the applicability of field theory to social phenomena. Unfortunately, both can become realities only through some of the methods he discards. Philosophers of history, including Mr. Zucker, in their scientific aspirations are still in the Natural-History stage of scientific development. In the end they will find that history, as such, will play about the same role in social as it plays in physical science. As in the other sciences, authentic history will itself be inferred from the laws of social behavior established by the accredited methods of science from contemporaneous data, subject to observation and generalization by these methods. It should be said to the author's credit that in some places (e.g., Vol. I, p. 33), at least, he seems to recognize this eventuality. Incidentally, the author devotes considerable space to an attempt to refute John Dewey's argument against the possibility of history as a science and, in my opinion, fails in his refutation.

The work closes on a Marxian note, although here as elsewhere the author shows considerable independence as, for example, in deploring the Russian political system while approving of its economic order. Also, he ventures to differ with official Soviet interpreters of Marx (Vol. I, p. 617). There is in fact much admirable objective analysis in these books, especially of recent and current history. Except for the scientific pretensions of the work, a more favorable review would be justified.

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DEVELOPMENT OF MEDICAL SCHOOLS

Medical Education in the United States before the Civil War. William Frederick Norwood. 478 pp. 1944. $6.00. University of Pennsylvania Press.

"MEDICAL education in the United States, with all its ramification, in the century before the Civil War, constitutes a significant and unique chapter in the social history of the country." This statement, with which this book closes,

expresses a conclusion that is thoroughly sound. The careful and laborious study that has gone into the making of this book throws much light on the development of an important factor in the cultural and social structure of this country. It presents many and, at times, tedious details of the struggles, often tempestuous, through which medical education fought its way. Rivalry, individualism, personal liberty, free enterprise without restraint, and struggles for existence, created many a battle between professors, between faculties and trustees, between rival medical schools and, at times, between the medical profession and the public, when protests against human dissection took the form of riots.

Organized medical education in this country had a dignified and promising beginning in Philadelphia in 1765, and progressed until the end of the 18th century under the leadership of brilliant young men who had studied medicine in Paris, in London, and especially in Edinburgh. By the end of the century four medical schools had been founded in connection with established colleges. These schools have all survived to this day as the medical schools of the University of Pennsylvania (1765), of Columbia University (1768), of Harvard University (1783), and of Dartmouth College (1798). Anatomical teaching, lecture courses, and teaching hospitals were planned and developed.

Soon after the turn of the 19th century an American system of medical education began to develop and to spread widely as social and political order of a sort extended throughout the broad reaches of our land. The general pattern of three years of apprenticeship with a practitioner of medicine, and the attendance on two short annual sessions in a medical school, to attend the same lectures each year, became the accepted requirements for the degree of doctor of medicine. Medical schools sprang up not only in the larger cities, but even in country towns, wherever five or six doctors who had a desire to teach could hold themselves together to form a faculty. These schools usually formed some loose attachment with an established college, often at a distance, and, in one notable example, in another state.

A number of influential teachers were itinerants and traveled about to form new schools or to join faculties that invited them from a distance. Some of them had a strong and lasting influence on American medicine. However, as standards of graduation were generally ill-defined, as the support of the schools, and indeed of the professors, depended upon the number of students that could be attracted, and as no licensing examinations were in force, American medicine was largely dependent on the natural ability, sincerity, honesty, and energy of those who practiced it. Fortunately these traits were by no means uncommon, though many practitioners without these qualities were turned loose on the public. American medical education during the 19th century up to the Civil War, as described in much detail in this book, does not present a phase of our history in which we can take much pride.

To the student of medical history, it is of interest to see fundamental improvements being made in scattered places, especially in the West and South. A notable innovation occurred in 1850 when the newly organized University of Michigan put its medical professors on a salary basis, similar to that of other University professors, and relieved them of their dependence on student fees. In 1857 the New Orleans School of Medicine introduced a system of individual clinical instruction of its students and gave them the opportunity to study the patient at firsthand. In 1859 Dr. N. S. Davis, who had taken the leading part in forming the American Medical Association about ten years previously, or-

ganized a two-year graded curriculum in the Medical Department of Lind University, Chicago. Thus, two distinct years of training were begun, instead of having students repeat the same course in two successive years.

The outstanding characteristic of this book is the thorough and complete historical study which lies behind it, making it a source of information on the development of medical education in the United States. Although much of the book is taken up with details of defunct medical schools that are not likely to interest the general reader, or even those especially concerned with American history, it is a useful reference book with much permanent value.

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APOSTLE OF THE WILD

Son of the Wilderness: The Life of John Muir. Linnie Marsh Wolfe. 364 pp. Illus. 1945. $3.50. Alfred A. Knopf, New York.

There is no more beloved name in the annals of the American conservation movement than John Muir. He was the evangelist of that movement, a crusader against man's wanton destruction of natural resources and beauty. He was a great naturalist, a pioneer geologist, a botanist, a lover of mountains and trees, more at home among the haunts of the water ouzel and wild sheep or in some alpine glacier meadow than in the "false society of men." Yet he lived among men too, raised a family, made many undying friendships, and left the world a better place. But for no one did he ever compromise his principles or fail to fight relentlessly for what he knew was right. He was, as his friend Bailey Millard said, as "wild as a Modoc and as unafraid as a grizzly."

All these characterizations, and dozens more that might be listed, make the story of John Muir's life anything but dull. His story has been told before—much of it in his own writings, which have become classic (*The Story of My Boyhood and Youth, Travels in Alaska, My First Summer in the Sierra,* etc.). The two-volume "definitive" biography, *The Life and Letters of John Muir,* by William Frederic Badé, appeared in 1923–24. But it is good to have Mrs. Wolfe tell it afresh. This author's interest in Muir, fortunately, is not a fly-by-night affair. She has been a student of Muir for many years, and in 1937 she edited a delightful book called *John of the Mountains,* made up of some of Muir's until then unpublished journals. Furthermore, she has had access to much new material, chiefly notes and letters and other Muiriana turned over to her by the Muir family.

John Muir was a many-sided individual; he lived a full and strenuous life. Some phases of his career would themselves fill large books. Mrs. Wolfe has done a good job of condensation of material and has produced an unusually well-rounded picture, emphasizing those things that show John Muir the man and setting straight a few matters that she believes have been misrepresented. Here is her thumbnail sketch of Muir:

"Far from being an effeminate plaster saint, all sweetness and light, as some of his admirers have conceived him, he was in truth red-blooded and intensely masculine; a mystic, yet a realist with his feet on the ground; a lover of solitude, yet gregarious and often a prey to bitter loneliness; frugal in supplying his own needs, but lavishly generous to others; a man of puckish humor and of stern, dour moods. Infinitely gentle and understanding in his friendships, and towards the young, the old, and all defenseless creatures, he was blazingly intolerant of bigotry and every form of social callousness. Although mellowed in maturity and more humorous in his judgments, he was still a fighter, an archenemy of all encroachments in the name of 'progress' upon human or animal rights."

In many ways the book is an expansion of this paragraph, and John Muir emerges as a great man. No one, it seems to me, could read it without gaining the highest regard for his genius, a new appreciation for what he did to save Yosemite, Kings Canyon, and other wilderness areas of America for the people, and a deep and lasting admiration for him. To me, some of the most appealing episodes of his story are those pertaining to his friendships—his meetings with Ralph Waldo Emerson and John Burroughs, kindred but contrasting spirits; his camping out with Theodore Roosevelt; his relations with Edward Henry Harriman; his love for the wonderful dog Stickeen; and many others.

The vitalizing thing about John Muir was his boundless and (to his friends) sometimes embarrassing enthusiasm. This was so ingrained in his nature as to be entirely unselfconscious. It was the urge that drove him time and again back to the mountains, that sent him traveling to the far corners of the earth, that kept him fighting and writing to the end of his life. He was a man who could be perennially renewed in spirit by communion with the living wilderness, who found fresh springs of loveliness and wonder with every recontact with nature. To read the life of such a man—so out of this world and yet so dependent on it—is a memorable experience and one that few biographies render. The book should appeal to many classes of readers—scientists, naturalists, conservationists, nature writers and poets, and especially to a newer generation of citizens, who may remember John Muir only by name but who are now enjoying more than they know the fruits of his persistent struggle to save vast areas of incomparable American landscape from devastation.

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GROWTH FACTORS

Bioenergetics and Growth. Samuel Brody. 1023 pp. Illus. 1945. $8.50. Reinhold Publishing Corporation, New York.

This book presents in detail the results of the researches sponsored by the Herman Frasch Foundation for Research in Agricultural Chemistry at the Missouri Agricultural Experiment Station.

Very few technical books contain sufficient experimental data and scientific developments to warrant a useful life of more than a decade. In this respect Dr. Brody's book is an exception to the general run of books. It is seldom that a book is published that will appeal to as many different fields as this one will. I bought the book for the chapters on aging, but found the book so well-written and so full of biochemical data that I read the entire book. The first part of the book (17 chapters) consists of a detailed, accurate, and careful treatment of experimental biochemistry, nutrition, and the biodynamics of life processes related primarily to farm animals but also containing a large amount of data on human problems. The chapters on hormones, vitamins, and enzymes are very complete. Throughout the book the kinetic approach and utilization of data are stressed. Dr. Brody has emphasized the economics of his work and the relation to agriculture in all of the material covered. The application of thermodynamics, as well as kinetics, to the interpretation of biological systems is to be advocated. The careful analyses of energy relationships will be of interest to the layman as well as the scientist.

The two chapters on aging are of especial interest to the workers in that field because they illustrate very well the fact that aging is the end product of two competing chemical rate processes in the body. The wealth of data on aging rates and the variation of physiological functions with aging are worth the price of the book.

The last part of the book (6 chapters) covers fundamental economic aspects of animal husbandry. Throughout the book the efficiency of various processes is emphasized and analyzed from the standpoint of maximum production, long-term production, and cost relationships. I have never examined a book of this size before that contained a greater wealth of experimental data in the form of graphs and tables. Some of the statistical methods used are given in detail and the mathematical results are developed from an experimental basis in many cases. The impression is given that the reader is following the problems discussed along with Dr. Brody and his associates during the years of work. The book is well illustrated with pictures. The make-up of the book is good and it is well bound.

Only a few typographical errors were noticed. The ones found were: p. 113, chlorophyll differs from hemoglobin in having different groups on the pyrrole rings as well as in the central metallic atom; p. 127, the formula for choline is in the wrong order; p. 137, oxidation and reduction may be defined as the loss and gain of electrons, but as a chemist, I cannot agree to the statement ". . . or loss and gain of protons;" p. 138, 924, the statement that fat is converted to carbohydrate in the body is disputed (The reference is old (1920) and has not been verified;) p. 144, the formula for 2-thiouracil is not quite correct; p. 150, parallel is misspelled; p. 158, typographical error, *only ln;* p. 283, the first graph should be labeled 11.11 instead of 11.11a; p. 926, in note 69, amphetamine is misspelled. Other minor errors will probably be found in time.

In the summarizing chapter available sources of energy are discussed from the standpoint of fuels. Atomic energy is practically discounted. Although wartime secrecy was in force when the book was finished (Nov. 1944), sufficient information had been published by 1940 to indicate the early development of atomic energy. Instead of having enough U235 for bombs in 1945, we would probably have had only enough to run an experimental power plant, but its early utilization was expected by many scientists. The omission of this point by Dr. Brody is a matter of opinion and should not be considered as a serious criticism.

This book is recommended to graduate students, investigators, and field workers in the fields of animal husbandry, biochemistry, nutrition, and aging. It will be considered a standard reference book for many years to come. Dr. Brody and the Herman Frasch Foundation are to be highly commended upon an excellent job accomplished.

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COMMENTS AND CRITICISMS

MORE ON ETHICOGENESIS

SCIENTIFIC ETHICS

I make no apology for attempting a further contribution on this subject. The atomic bomb underlines the significance of the discussion which has recently taken place in the pages of THE SCIENTIFIC MONTHLY. Science has given to the world an instrument of stupendous power. Thereby comes a challenge. Will science provide us with the means to control that instrument in the interest of humanity or is it to become a Frankenstein monster to encompass the destruction of its creators? Are scientists concerned with ethical questions, or is this the preserve of the philosophers? Whatever the answer, scientists are also human beings and as such have a vital interest in the ethical question whether it is to be solved by scientific or philosophic method.

Right at the outset of our discussion I think we should try to break down the exclusiveness of science and philosophy. Dr. Leake contrasts the descriptive thinking of science with the normative approach of philosophy with its "pure reason." Dr. Romanell pertinently explains that this applies to aim rather than method, but I think there is more to it than this. Probably most people would agree that philosophy has tended unduly to armchair thinking inadequately checked at the descriptive level while science has ignored the very existence of certain fundamental problems. The basic distinction, however, is wideness of aim. We have had sciences in the plural but there can be only one philosophy. True there may be rival claimants for the honor but that is a different matter. The various sciences may be equally acceptable and mutually compatible. For the most part, however, it has been impossible to judge this compatibility owing to the exclusive nature of the fields. But of late years there has been a tendency for some of the sciences to overflow their boundaries and link up with others, as in the case of chemistry and physics. We may expect more and more of this with an increasing tendency towards the building up of one integrated science.

It is this movement towards integration which leads the scientist into the field of philosophy. What is to come of it? Will the new science supersede the old philosophy, or will it fall short of philosophy in respect of aim or method?

Let us consider method first. Essentially there is only one method to develop understanding. It consists in gaining data and seeing its interrelationships. We are continually getting data through our senses but sensations mean nothing until they are interpreted. When Kohler's ape broke a branch from a tree to use as a stick to draw bananas into his cage, it was the result not merely of his seeing the tree or the branch but seeing the relation of the piece of branch to the no-stick-to-drag-in-banana situation. Now the charge against the philosophers is surely that they have gone on weaving a web of these relationships until they have achieved a comprehensive theory which solves their philosophical problems but has not kept in touch with objective data en route. Scientists, on the other hand, while making a great show of meticulous attention to objective details have failed to notice that they have also made some sweeping assumptions as the basis of their interpretation.

The upshot of all this would seem to be that the time has come for scientists and philosophers to develop a greater degree of cooperation.

Secondly, we have to consider aim. Romanell opposes the existential of science to the normative of ethics and finds that philosophy is aiming at "wisdom" whereas science aims at "knowledge." Presumably no matter how wide may become the field of scientific knowledge it fails to equate itself to

the "wisdom" at which philosophy aims. This latter is concerned not with what is but with what ought to be.

Here seems to be the real crux of the problem. Here is the ground for asserting an essential difference between science and philosophy. Here we become faced again with the problem of what does constitute a basis for the laws of ethics.

This distinction as between the existential and the normative would seem to imply that ethical data is in some way less real than that of science. It does not really exist. It is just a sort of plan of what might and should exist. It is concerned with laws which derive their validity from something we know not what, whereas the laws of science are such in their own right!

It seems to me that the behavior of man through the centuries reveals a deep-seated resentment against this. He has strived in various ways to justify the fundamental claims of moral law. All this may be explicable in terms of psychoanalysis, or maybe psychoanalysis itself uses assumptions which equally need explaining. Any psychology must have some theory of motivation. With regard to human conduct we can justifiably ask "why?" in a sense unknown to the scientist as such. You may ask the scientist why it is raining but his answer is almost certain to be in terms of "how." He will trace for you a set of relationships in time and space, but when mother asks Tommy why he stole the jam, she is not satisfied with an explanation of this kind. Ultimately the answer to "why?" must be in subjective terms. A man doesn't really make love because it is conducive to the welfare of the species; he does it for the unique satisfaction which it gives him. He does it because the activity has value for him and value is something experienced only subjectively.

I would suggest that all our human behavior, or at least what we regard as the "higher" forms of behavior, are determined by a set of values of this kind. These values, I would hold, are just as real and fundamental as the qualities which we assign to matter. After all, matter is just the common-sense interpretation of our experience. Our evidence for value is of the same nature as our evidence for electricity, and perhaps even more direct.

If we can accept value in this sense, we shall not be called on to explain ethics as based upon some attenuated theory of reasoning. No one can be motivated by pure logic; it must always strike home to some value complex if it is to influence behavior. We all feel the truth of that in some way but difficulties arise because we think of human desires as essentially selfish and often at variance with what we regard as good and right. We are thus led to look for an ethical standard which transcends human desires. The biologist naturally tends to think of this higher standard in terms of racial welfare, evolutionary progress, increasing survival value. He fails to notice that, if matter is subject to evolution, value may be subject to the same process. More especially he fails to notice that this concept of value, far from emphasizing the selfishness of human beings, provides the bridge by which individual action may lead to group welfare. The behavior which value motivates is not necessarily that which will further individual welfare. It may dictate the sacrifice of the individual for the benefit of others.

Scientists such as Leake make the fundamental mistake of thinking that individual welfare is all-important to the individual in its own right. Not mere living is the important thing but the experiencing of values, and we might have the paradoxical state of affairs where dying provides the greater experience of value. But notice that individuals so constituted would hardly live long enough to reproduce their kind. Evolving values must be, on the whole, conducive to the survival of the individual and the race so that values do subserve racial continuance, and thus Leake's criterion seems to be justified. We have just introduced a further term. But have we? Might it not be more logical to assert that we continue life in order to experience value than to say that we experience value in order

to continue life? For we cannot legitimately talk in terms of purpose and desirability until value exists, and the fact that value promotes the continuance of life cannot be used to prove that life itself is of greater value than the value which informs it.

All this may seem at first rather confusing and, apart from the paradox, might be expressed more clearly, but once the nature of value is understood all the rest follows.

Finally let us examine in more detail the problem of the individual acting in the interests of the group at the cost of his own welfare. This is the rock on which so many ethical systems have foundered. Some systems have established a satisfactory goal in terms of group welfare but have been quite unable to provide any adequate motive for its pursuit; others have found adequate motivation but have been unable to direct it beyond enlightened selfishness. Morality is surely something more than the latter. The concept of value as I have propounded it here allows us to exalt ethical principles to a place of honor without any need for mystical derivation of the authority for moral law. Moral law, like physical law, is implicit in the evolving universe and needs to be derived neither from the personal commands of a deity nor the logic of the scientists and philosophers. Logic may indeed be used in its discovery, but this is rather different from invention, which is more nearly the role that many scientists would seem to think necessary.

All life processes are necessarily associated with value. That which conduces to the survival of the individual, by a process of evolution rather than of logic, be it noted, tends to be linked with value, so that what individuals wish to do is usually in their own interests. But that which tends to race survival is necessarily even more certainly linked with value, so that in case of value conflict, which is very frequent in some form, the racial values will tend to predominate over the individual values. So individuals are able to transcend what we might term their inherent selfishness. We are capable of a divine nobility.

I should like to pursue my theme into some of its many ramifications but I will content myself with one more effort to drive home my fundamental point. I wish to emphasize that value as I have spoken of it here exists in its own right. It is one of the fundamental givens. Certain laws of thought can be denied only if we assume their truth in the argument by which we deny them! Value would seem to be in a similar position but in a more subtle way. Within the realm of conduct we take value for granted whether we recognize it explicitly or not. But if we do recognize it explicitly as a fundamental it provides us with a key to unlock our ethical puzzles. We can proceed to study the evolution and integration of values and thereby explain the normative problems in terms of a wider existential basis.

C. J. Adcock

A SCIENTIFIC VERSUS A METAPHYSICAL APPROACH TO ETHICS

The response to my provisional attempt at "ethicogenesis" in the April Scientific Monthly has been startling and stimulating. It ranges from querulous versifying to Professor Patrick Romanell's patient and helpful philosophical analysis in the October Scientific Monthly. Most gratifying has been the impression that it may be possible to promote a cooperative endeavor between interested philosophers and scientists in exploring a proposed scientific approach to ethics.

While many professional philosophers are frankly skeptical of approaching ethics in a scientific manner, there are apparently some who are interested, especially in attempting a study of the consequences of the evolution of interhuman adaptations. On the other hand, many seem to insist on retaining an *a priori* metaphysics (often supernaturalistic) as a basis for a theory of ethics. Even while admitting that the principles of ethical theorizing may be as scientifically developed as in physical theorizing, they claim that the subject matter to be dealt with is not of the

same sort. The "primitive facts" for ethics are held to be individual judgments such as, "This act is wrong" or "This man is evil" or "This ought to be done." It is the scientist's contention, however, that the primitive facts for ethics are the same as those with which scientists usually deal; namely, the elucidation of the consequences of action (or energy exchanges) on the basis of our objectively verifiable and voluntarily agreed upon knowledge of ourselves and our environment.

The resolution of the difficulty may occur through careful consideration by philosophers of what demonstrable knowledge we now possess of the mechanisms of brain activity. Instead of continuing to keep their heads ostrich-like in the barren sands of centuries of speculation on the semantic mysteries of "mind," some of our philosophers might profit from a careful reading of current neurophysiology.

While there is fair general appreciation of the work of Pavlov, Head, Sherrington, Cannon, and Herrick, there is little evidence of significance of the work of these men in the writings of the majority of current philosophers. One would think that philosophers would base their speculative ideas on voluntarily agreed upon and objectively demonstrable facts. It may be too much to ask philosophers to keep abreast of current work in neurophysiology, but at least one might expect them to become acquainted with the classics in the field. It should not be expecting too much to ask them to familiarize themselves with recent summaries, such as those of Tilney, Krieg, and Fulton. Philosophers might also derive much interesting but inconclusive information regarding the chemistry of the brain from Page's book by that name.

As study of the brain proceeds, scientists expect the disappearance of the notion of "mind" as something supernatural and thus different from the possible mechanisms of matter. Now that the expenditure of two billions of dollars has resulted in the awful control of the release of atomic energy, with the possibility of a controlled peace, it might be wise for us to devote at least an equal sum to the study of energy transformations in the brain. We might thus learn what limitations there may be to the validity of our thought.

With reference to Professor Romanell's "reply," let it be clear at once that it is generally agreed that philosophers have the responsibility of developing our rapidly growing scientific knowledge into such "faiths" or "syntheses" or "wisdom" as may be satisfying from time to time to our yearning to understand "what it's all about." In undertaking this important responsibility, however, it seems necessary that philosophers keep within the limits of what our growing scientific knowledge shows to be possible or probable.

In a tender discussion of "Mathematics as an Intercultural Bridge," Arnold Dresden ably refutes the common notion as expressed by James Darmesteter that "science equips man, but does not guide him . . . it is invincible, but indifferent, neutral, unmoral." Scientists may strive to be neutral, but never indifferent nor unmoral. Dresden shows that the basic mathematical principles of inversion, of the order in which operations are performed, and of existence theorems are of profound moral significance, under conditions of reasonable association. Similarly other scientific endeavors in physics, chemistry, and especially in biology may be shown to have significant moral implications under the same conditions.

Spencer indicated these contributions of science to ethics, as did Leslie Stephens, W. R. Sorley, A. E. Taylor, and George Gore in a previous generation, and as Dewey and his pupils, R. B. Perry, Conklin, Herrick, Holmes, Cannon, Waddington, Julian Huxley, and others are doing now. It seems prerequisite to a discussion of what constitutes right or wrong conduct to elucidate, on the principle of inversion, all possible consequences of action, whether particular or general. Further, for such a discussion it appears necessary, on the basis of an existence theorem, to determine what conditions are essential or sufficient for a desired end or

purpose. These prerequisites can be met in part by the efforts of scientists. The synthesis or coordination of agreed upon scientific data, with reference to a general problem or proposition, is the business of philosophers.

The Greeks appear to have recognized the trinitarian relations of logic, aesthetics, and ethics. They emphasized the necessity of knowing the "truth" about a matter before purposes can be chosen, or techniques used to apply knowledge to the accomplishment of purposes. The job of scientists is to obtain "truth"; of philosophers and statesmen, to choose appropriate and possible purposes, and of artists and engineers, to apply knowledge to the accomplishment of purposes.

Professor Romanell pays me a great compliment in so carefully analyzing my effort to induce a naturally operating ethical principle. His analysis reveals many points at which I failed to make myself clear. Thus, I had hoped that my definition of the general procedure of scientists would have indicated the importance of the experimental endeavor as well as of the descriptive approach. My emphasis on the latter was made in contrast to the normative approach characteristic of classical ethics. My purpose was to suggest the possible importance of a scientific approach in ethics, particularly from the standpoint of elucidating the consequences of actions and the conditions necessary or sufficient for the accomplishment of purposes.

It is not clear that ethics by definition is essentially a normative pursuit. Ethics is concerned with the problem of goodness, which is a matter of conduct, and thus of relations between people. Sociology, anthropology, psychiatry, psychology, and thus in a broad way, physiology and biology, become significant scientific disciplines for ethics. Nor are the raw data for ethics merely the judgments of different persons as to what is right or wrong, or what ought or ought not to be done, as Ducasse implies. The various factors conditioning these normative concepts are perhaps even more important as data for ethics. Such factors are elucidated by scientific effort.

Professor Romanell says that a normative inquiry is concerned mainly with studying objects as they ought to be. Is it not pertinent first to explore, as in an existence theorem, what is possible for these objects of study to be? Such an exploration may proceed with greater surety by scientific methods rather than by speculative. If "ethics is an examination of the *good* life," then it must also be an examination of the factors in life which are conducive to the good. Such an examination would surely profit from a scientific approach, both descriptive and experimental.

There seems to be some confusion in Professor Romanell's logic regarding existential and normative inquiries. If a normative inquiry is concerned with what ought to be, then it certainly involves a method of choosing what ought to be. If this method is speculative or metaphysical, as in classical ethics, then it may fall to pieces by failing to recognize the impossible. If the method is scientific, one proceeds, first, by knowing what is, and then, second, by knowing the consequences of what is. When the consequences are known, an adaptive or rational choice may then properly be made with respect to the purposes intended. Rationality in choice involves appropriateness of adaptation to the end desired. The normative inquiry involves, as Romanell implies, choice of purpose. However, in order to have any validity, that choice must be based on what we know is possible. It seems, therefore, that there is a normative method in ethics, which, if not properly replaceable via scientific method, might at least well be supplemented by one. To this, I think, Professor Romanell may agree.

To the scientist it is irrelevant or meaningless to discuss in general how people ought or ought not to act. The scientist proceeds by finding, through verifiable experiment, the particular conditions necessary to be met in order to achieve some particular end. Our effort to generalize this proposition results in the tentative formulation of an operative natural principle, inducible from the plethora of common experience, to the effect that, if

the end to be reached is a lasting, satisfying relationship between individuals or groups of individuals, the condition necessary to be met is that the relationship must become mutually satisfying. It is perhaps significant that none of the many critics of my essay on ethicogenesis make a systematic attack on the attempted formulation of a naturally operative ethical principle.

Professor Romanell is also confusing in his discussion of the relation of the real to the ideal. If the ideal is distinguished from the real, then its unreality is admitted, and its validity becomes questionable. Is it not possible to place the concept of the ideal in appropriate scientific reference by considering it to be an asymptotic limit, never to be reached, and therefore valid merely as an asymptote?

It is regrettable that I should have implied to Professor Romanell that philosophy is dead, when I suggested that perhaps metaphysics has no further meaning in view of our present knowledge of ourselves and our environment. It would seem, in the light of current scientific developments, that philosophy might advance more rapidly and effectively without metaphysical impedimenta by deriving justifiable generalizations from the demonstrable scientific knowledge we now possess of ourselves and our environment and by elucidating the consequences thereof.

Professor Romanell might be under much strain if called upon to justify claims for an inverse relation between certainty and depth of knowledge, or between detail and significance of knowledge. If he was thinking of the Heisenberg uncertainty principle when he thus dogmatized, he was unfortunately led into a false and dangerous analogy.

Many philosophers make much of the notion that an organism is more than the sum of its parts. This is perhaps not a fair way to state the proposition. There is the assumption that an organism appears to be more than the full sum of all of its parts and their respective potentialities. Some of the available data on biological levels of organization have been summarized by A. E. Emerson and R. W. Gerard. The factor of time in the organization of living things may also be explored scientifically as du Nouy showed, with more than a hint of metaphysical skill.

There is nothing occult, metaphysical, or supernatural about the concept implied by the word "wisdom." Neurologists indicate that richness of sense experience may be readily reflected in richness of neuron association. Similarly "vision" and "understanding" are symbols referring to the ease and richness of neuron association in the brain in bringing together appropriate neuron association pathways built from the richness of sense perception and experience. By reference indeed to the degree of universality of neuron association pathways in different individuals as manifested by similarity of reaction to similar sense experiences, one may apply the canons of science to philosophical propositions. Those which may be refuted by this method may properly be termed metaphysical irrelevance.

Professor Romanell apparently does not differentiate between philosophy and metaphysics or between metaphysics and science. To the scientist, however, it seems that metaphysics has no validity because it has no basis in the objectively verifiable or demonstrable. This is to say that it has no basis in scientific knowledge, and hence no present reality. Philosophy has a valid and significant function in coordinating scientific knowledge and elucidating the consequences thereof. An attempt was made to define scientific endeavor in the article discussed by Professor Romanell. By metaphysics is generally meant that which is beyond the physical. The scientist can find nothing there, and hence can't believe in it. In my discussion here I am using "metaphysics" as a symbol for all supernatural explanations or speculations about phenomena, and as a general symbol for such presumed nonphysical or nonmaterial, but allegedly real, concepts as "mind" or "soul."

It is from this position that the scientist rejects metaphysical irrelevance in attempting an approach to ethics. As Professor Romanell proceeds in his discussion of the

interrelations of science and philosophy it would seem that scientists generally would be in sympathetic agreement with him. The scientist holds, however, that the "truth" can best be obtained by the scientific method and that metaphysical considerations introduce unnecessary nonverifiable speculations and semantically confusing concepts.

In his critique of my Darwinian argument in connection with a naturally operative ethical principle, Professor Romanell reveals again my failure to make myself clear. I attempted to emphasize the significance of adaptation in the Darwinian exposition of evolution. Darwin realized his error in referring to a "struggle for existence." It was this phrase which Nietzsche exploited and against which Samuel Butler, Edmund Montgomery, Henri Bergson, Alfred Noyes, and Jacques Barzun so vigorously reacted. It was Thomas Huxley who unfortunately popularized it and himself revolted from it. But Charles Darwin did not stress the struggle for existence nor "nature red with tooth and claw," nor the "gladiatorial theory of existence." Darwin did emphasize adaptation to environmental circumstances as a necessary condition for survival. It is this aspect of Darwinian evolutionary theory which is considered to be significant ethically, and which I think he so regarded.

An attempt was made previously to be explicit regarding the implications of Darwinian factors in survival, but apparently Professor Romanell preferred to follow the usual but rather erroneous ideas associated with Darwin's thesis. His strictures against my point of view are therefore slightly irrelevant, but they are helpful in proposing a clarification of the matter. It is quite unfair to Darwin to imply that Nietzsche's iron rule is part of Darwinian ethics. Darwin's contribution to ethics was essentially to offer biological evidence in support of the Aristotelian harmony theory of ethics. This, as has been later developed, appears to be a mater of adaptation, adjustment, and compromise toward what may become mutually satisfying.

My very minor contribution is simply to offer expression of the conditions under which relations between human individuals or groups may be expected to survive. This expression gives a scientific basis for choice of conduct, and thus for ethics. From the plethora of universal experience, this general principle may be induced: Behavior patterns between individual persons or groups of people tend to become adjusted (by trial and miss) toward those which yield the greatest mutual satisfaction. It seems that the concept of "good" arises from developing experience with the more satisfying behavior pattern.

The principle may perhaps be more scientifically expressed in a manner explicitly stating the conditions under which relationships between people may be expected to survive: The probability of survival of a relationship between individual humans or groups of humans increases with the extent to which that relationship is mutually satisfying. As a practical matter, this principle may be worth much attention and appropriate application in dealing with our Russian ally. The Russians are perhaps most likely of all peoples to appreciate the natural operation of the principle.

To Professor Romanell's inspirational closing it may be asked, If all good people sacrifice themselves, what happens then? We can't all be Jesus Christs, John Husses, Giordano Brunos, Miguel Servetuses, or even Marines. Some of us have to go on living ordinary, commonplace, everyday lives. Perhaps it even takes more heroism to face the dull grind of routine, humdrum daily life with equanimity and dignity than to ride the winds of fancy snatching at what seems to be fire in hope of being another Prometheus.

Let it be hoped that this discussion may be extended to a serious effort on the part of philosophers and scientists to understand each other more thoroughly. Scientists certainly have much to learn from philosophers, and it seems that philosophers might profit greatly by becoming more fully acquainted with current scientific developments, par-

ticularly in biology, neurophysiology, psychology, psychiatry, anthropology, and sociology. The efforts of scientists in these fields may supply significant and important data not only for a sound ethic, but also for a satisfying general philosophy.

CHAUNCEY D. LEAKE

CRACKING THE CONCEPT

The wish to live in a more decent world is felt by man without the least effort on his part. The preference for better conditions in the presence of inferior ones is inborn in man. This is what John Dewey has in mind when he remarks in his *Human Nature and Conduct:* ". . . the ideal is itself the product of discontent with conditions." But this wished-for better world will not become a reality without our effort—a long, sustained, concerted effort.

However, in order that man may be persuaded to put forth the intense effort required to change chaos into order, he *must* feel that he has the necessary stature for the assignment, at least the potentialities.

The man Christian philosophy looked at had the required stature for the task. But the Christian concept of man has disappeared from academic circles.

The naked statement that the Christian concept of man has vanished from Academia may be shocking to some. But this is what Mortimer Adler is trying to tell us when he writes a whole book on what man has done to man: when he writes on what psychoanalysts, psychologists, psychiatrists have done to man's concept of himself.

Let us see how much the modern educated man has wandered away from the Christian concept of himself. According to the Christian philosophy, man's reason—"the soul's summit," as Thomas Aquinas called it—has a higher hierarchy than the rest of man's hungers. The rest of his hungers are those which he has in common with the beasts. This is so definitely true that the entire Christian concept of freedom rests squarely upon the Christian concept of reason.

Hierarchy, however, is a concept alien to the so-called scientific approach. Let us look at this reality through Professor Stace's book, *The Destiny of Western Man.* I choose this book because it received the sole award of $2,500 in a contest intended primarily for educators, and because the committee which made the award was composed of prominent men. One of them was Carl Van Doren, who called it "a book of world-wide significance, sure to clarify and fortify contemporary opinion and to leave its mark on years to come."

Says Professor Stace on page 93 of his book: "The Greeks, therefore, had in general no right to their belief that man is superior to the other animals. . . . And therefore, we cannot admit the validity of that argument in favor of the *primacy of reason* which bases itself upon man's superiority to the rest of creation." (Italics mine.)

However, unless the concept of the primacy of reason is recovered, even John Dewey is meaningless in education: "The ideal aim of education," Dewey says, "is the creation of power of self-control." Self-control, we are tempted to ask Dewey's pupils, is control of what over what? What is self identified with? Hierarchy is implied in the term self-control. Or else the term is meaningless.

That the term self-control is meaningless at present is revealed by the unsubstantial, makeshift character of our education. Quoting "someone," Professor Frederick G. Nichols of Harvard wrote in 1942:

> About 1900 in the University of Iowa a teacher took a hen into the class and, while this was a good deal of an innovation, it was simply a hen. About 1910 this hen had become a "problem." About 1915 it had become a "project." About 1919 this hen was a "unit of work." Around 1925 it was an "activity." In 1930 it became the basis of an "integrated program." And lo! In 1936 this poor hen has become a "frame of reference."

In our age two philosophers—especially two—have taken the warpath to recapture the Christian concept of man: Jacques Maritain and Mortimer Adler. They are trying to recapture the concept via Saint Thomas

Aquinas. They have taken the silent, solemn road untrodden by modern specialists. I, for one, would like to see them come out victorious in their daring adventure, but I do not even dare hope for their victory. Borrowing Korzybski's terminology, one would say that our age has become so "neuro-semantically conditioned" by education as to have become "constitutionally incapable" of understanding such men as Aquinas or Kant. On page 140 of his *Destiny of Western Man* the Princeton Professor says: "And this outward aping of goodness was what poor Kant thought to be the essence of virtue." After reading such a statement where aping of goodness and Kant are uttered in the same breath, and after taking into consideration the authority conferred upon the statement by the award, one feels that both Maritain and Adler had better take notice of the closed-road signs along the solitary path which they have been traveling.

It may appear absurd to philosophers, but in our age of specialization it is not only man's concept of matter which must come from science, but also man's concept of himself.

The much-discussed release of nuclear energy might afford the opportunity for scientists to step forward with a concept of man which will make integration of knowledge possible. Knowledge is disintegrated simply because the concept of man is out of joint. What disintegration of knowledge really means is that it is not culturally mandatory to make good use of knowledge. However, if the scientific concept of man coincided with the Christian concept of man, it would be culturally mandatory for man to make good use of his knowledge. Once man is put together everything else falls into place. And now that the scientists have released nuclear energy, the layman feels that science is responsible for the giant's behavior. And science would be equal to the task of controlling the monster if it had evolved by now a descriptive ethics of its own. Men of science talk and talk about the necessity of a descriptive approach to the subject of morality. Yet they have not given us that descriptive ethics which should replace the normative variety which has been so bantered and discredited.

A descriptive ethics must of course describe man, not his doings. This last would be history. And it cannot take the easy and superficial course of describing man as a "domestic animal." It has to describe the *specifically human* nature of man, not the part which man has in common with the beast. It must take account of this fact in man's nature which was pointed out by Kant: "Everything in nature works according to laws. Rational beings alone have the faculty of acting according to the conception of laws, that is, according to principles. . . ."

At the stage of specialization of our knowledge, to determine what is specifically human in man requires a veritable cracking of the concept of man. This cracking, in its turn, requires a concerted effort of specialists; as much as was required for the atomic bomb.

Let us glance at the following three fields of specialization to realize why the effort must come from many fields:

Psychoanalysis. Here is a school which denies rationality and puts rationalization in the place which rationality leaves empty. If the psychoanalyst is analytic enough, however, he soon discovers that his school cures by the very opposite principle which it proclaims. No psychoanalyst cures, for instance, by providing suitable mates; he cures by providing explanations. And a person who can be cured by explanations must be a highly rational being. An individual for whom to understand is to heal must have a diffuse rationality—one which penetrates and overpowers libido.

While the psychoanalyst is curing by explanations he could jot down for his descriptive ethics: Once reason understands it creates order below. Reason, therefore, must have an immanent power to govern the psychophysical hungers of the self.

Hypnotism. Of course a specialist could remain at the toying stage with hypnotism. But a specialist might heed Hegel's words: "I only know of an object insofar as in it I also learn of myself." If he tries to

learn of himself, of man, in his object or subject he may learn a lot. His experience has shown him, for instance, that under hypnotic or posthypnotic suggestion a man will dispossess himself of all his worldly goods and bequeath them to a stranger. Should the reader be skeptical let him consult Hugo Münsterberg's authoritative *On the Witness Stand.* And in some cases a man under posthypnotic suggestion will even accuse himself of a crime which he has never committed, and thereby land in the electric chair. But a man under hypnotic influence will not violate what to him is a moral principle.

When this something which is stronger than hypnotic suggestion comes to the surface of man's being, the specialist could write down for his descriptive ethics: Man's affinity for a moral principle is stronger than his instinct for self-preservation.

Industrial Psychology. The long experiment conducted at the Western Electric Hawthorne Plant reveals that man as a physical organism does better under bad conditions than under unjust conditions. Only a highly rational being would be affected by unjust conditions which are physically good. And only if rationality has an immanent power over the rest of man's reactions would the above characteristic reveal itself under experimental conditions.

The descriptive ethics which could come from cross-specialization must establish these two facts: (1) Man's rationality, differing not only in degree but in kind from animal intelligence. (2) Reason's immanent power to govern the rest of man's personality.

These two facts must be proved scientifically if democracy is to be differentiated from nazism by something more than military victory. Democracy is a product of a theory of human personality; nazism is also a product of a theory of human personality. What is the personality behind the democratic theory? Well, democracy is a faith that man can live with self-imposed oughts. Only a rational being, of course, could live with self-imposed oughts, and only so if his reason has an immanent power to govern his desires and impulses. Otherwise oughts would have to be externally imposed.

If man's affinity for correct solutions were not potentially stronger than his affinity for the things which satisfy his cravings, democracy would be a dream. Democracy would stand distant and empty—a meaningless Greco-Christian myth.

In writing their descriptive ethics, specialists must not forget that man could have been so constituted that he had an affinity for wrong solutions, or at least that between a right and a wrong solution he would be indifferent. Had that been man's condition, science, philosophy, and democracy would be unknown in the world. Democracy would also be impossible if man did not have a wider scope for perceiving as right the right solution than for discovering it.

Science has steadily rejected normative ethics. But in this hour of crisis, in which science is about to lose its freedom, would not scientists gallantly step forward and give us a truly descriptive human ethics—an ethics that could not possibly be thrown off the saddle by scientific progress?

To match the cracking of the atom there must be a parallel cracking of the concept of man. Each specialist would have to start bombardment within his own field. If he goes at it hard enough he will discover, crouching within his own field of specialization, a man of much larger stature than the one his specialty stands for.

If the bombardment is started from all quarters and the concept is cracked, the release of spiritual energy will be voluminous enough to make physical nuclear energy behave. It might be powerful enough to light the lamps of peace and keep them burning.

Ana María O'Neill

Prof. Sumner's Wise Reflections

Permit a mere journalist, retired after fifty years of activity, and now facing extreme old age, to congratulate you on the publication of Prof. Sumner's profound and philosophical article on old age and death. War almost invariably leads to the revival of what some people choose to call religion, but what is in reality crude superstition and naive beliefs supported by no evidence worthy of the name. These efforts are futile and vain, of course, but while they last, they do some harm. Voltaire did not say that those who believe in absurdities are prone to commit atrocities, but he might have said it, for it is true.

The dread of death, as Prof. Sumner—who, by the way, was a fellow La Jollan and almost a neighbor of mine—points out, is either a dread of hell-fire, which is an absurdity, or else a dread of total extinction, and this kind of dread is normal enough. Huxley had it, as he confessed. But we are intelligent beings, for the most part, if we get any education and introduction to the wisdom of the ages, and we learn that extinction is the common lot, except the lot of matter in its simplest form. Human life is tragic, because life abhors the idea of extinction. But life, on the whole, is good, not bad, and it is better to live the average human span than not to live at all.

As for individual or personal immortality, it is what Spencer called a "pseud-idea." No one can imagine the existence of a disembodied soul or spirit. No one can really believe that after death something indescribable and inconceivable leaves the body and continues to float somewhere in space in some other shape or form—doing what, serving what purpose, pray? No one knows. No one *can* know, apparently. Can we take seriously those who tell us, solemnly, that in heaven there are cocktails sans alcohol and tobacco sans nicotine? (Sir Oliver Lodge's "Raymond" for instance?) Prof. Sumner's reflections on this question are criticism-proof.

In recent magazine papers, some writers have taken the odd position that immortality, which cannot be proved, is at least "probable." Why? Because, forsooth, nature cannot afford to waste and lose human personality, the most precious thing there is in the universe. The conservation of matter, or energy, suggests that personality, too, is being conserved. This argument is not even plausible. What becomes of personality in senility, in loss of memory as the result of certain diseases—as in the case of Henrik Ibsen, who had to relearn the alphabet after seventy? What happens to personality in the case of the young who die *before* maturity and full development of their powers? Finally, the best in personality does survive death, not in the shape of a disembodied soul, but in the form of books, pictures, music, buildings. Bach, Beethoven, Dante, Milton, Shakespeare, Kant, are not dead—they live and mold our lives, ennoble and enrich our personalities.

It is to be hoped that Prof. Sumner's brave and candid paper will encourage other men of science to publish their ripe reflections on life, senescence, death, and reasonable ideas of immortality. Men of science are leaving their ivory towers, or their laboratories, and manifesting concern about our pressing and difficult problems, moral, economic, and social. It is their duty and privilege to help build a better world, a world free from superstition, barbarism, and injustice.

VICTOR S. YARROS

Professor Sumner's Unwise Reflections

In the August 1945 SCIENTIFIC MONTHLY the essayist of "A Biologist Reflects Upon Old Age and Death" has certainly ventured far afield from his entitled theme. He has virtually tried to offer a new concept on suicide, and one on the status of religion in the realm of "thinking persons."

With a view to our own laws and moral codes, (which we observe and cherish as American) suicide is an illegal act, is immoral, foolhardy, and cowardly. Incidentally, we certainly should not and cannot evaluate nor judge the Japanese with their national propensity for suicide, by our own laws and moral codes. But the author records, "Suicide, far from being 'cowardly,' as is so often pretended, is for most persons an act of supreme courage. Is it not really cowardice which has prevented most lives from being ended prematurely at moments when conditions seemed intolerable?"

It is disconcerting to observe a "scientist," one who searches for the truth, expounding a self-styled doctrine which, when carefully and impartially analyzed, is neither scientific nor popular.

The author's subtle thrust upon religion is the equivalent of an explanation for his neat compilation on suicide.

Otherwise, the orderly expositions on lack of driving power, becoming less impressionable, and the evidence of a failing memory seem to be more germane to the implications of the title of his interesting paper.

The author's inference, as expressed in his footnote, "that scientists as a class are 'tough minded,'" is indeed presumptuous. Any limited circle of friends or acquaintances should not be used as an indiscriminate "yardstick" by anyone.

HARRY MAETH, D.D.S.

THE BROWNSTONE TOWER

Alexander Wetmore, Secretary of the Smithsonian Institution and a distinguished ornithologist, was kind enough to comment on our Christmas story of the owls of the Brownstone Tower. He wrote as follows: "The barn owl of the Smithsonian towers is an old friend of mine, as I have known this bird, and his predecessors in tenancy, since 1912. Actually, the bird has become almost a legend, since Pierre Louis Jouy, who worked at the Institution for a time, records this owl in his list of District species published in 1877, and Coues and Prentiss in 1883 remarked that it 'is known to have nested in the Smithsonian towers'."

In a later conversation Dr. Wetmore told me that the owls nest in the small tower at the northwest corner of our building. From my chair I can see one of the owls' doorways —an open quatrefoil above a pair of narrow windows. One time the quatrefoils were screened without Dr. Wetmore's knowledge. Upon learning that the owls had been dispossessed, Dr. Wetmore ordered the screens removed. The birds soon returned to their rent-free home and, we suppose, have lived there happily ever since.

Another friend who enjoyed the owls added to his compliments: "Wish you would write more about your general philosophy of things." It is a temptation that I shall resist as long as possible, for the SM already contains the wisdom of wiser men than I. If I should attempt to preach from the Tower, I fear that I would be taking unfair advantage of those who do not have such a pulpit, and I might even come to regard myself as an oracle. Therefore I would rather risk the faults of triviality than those of profundity. Let me leave the latter to the owls.

Those birds are not the only fauna of the Tower. I could tell the story of the recumbent roofers, but perhaps I should confine my remarks to the so-called lower forms of life. Take the starlings, for instance (and you're welcome to them). Fortunately, only a few of these birds flutter about our windows, though myriads roost on the ledges of the Archives Building across the Mall. Could it be that our owls are protecting us from these desecraters of monumental buildings? Dr. Wetmore says that starlings are their principal prey in winter.

We have no "Archie" to criticize us. He and a few of his fellow-cockroaches live in the basement of the Smithsonian, where he has assisted Dr. Earl S. Johnston as a subject for tests of a sensitive method for the determination of minute quantities of carbon dioxide. After taking many deep breaths for Dr. Johnston, Archie has returned to his friends and relations, none the worse, we hope, for his exhausting experiences.

In referring so familiarly to insects I hope that I have not offended one of our readers, an engineer, who wrote that he wanted the SM, provided we would not publish any more pictures of bugs. An entomologist tends to forget that cockroaches are almost as repellent as snakes to many people. Perhaps it is safe to write about an insect not so well-known.

One night this winter, while my wife was reading, a hemipterous bug about a centimeter in length walked slowly across her book. This insect was garbed in scarlet and black, the colors of my first college, Haverford. It seemed bewildered on the printed page, so I gently brushed it aside, explaining that it was harmless—only a box-elder bug seeking shelter for the winter. For weeks we saw one bug every night ana liked to think of it as the same individual, whom we called Elmer. He was a friendly fellow but finally he took advantage of our tolerance, for he called in "his sisters and his cousins, whom he reckons by the dozens," and we had to take steps to curb the invasion arising from the cellar.

F. L. Campbell